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THE OPTICAL TREATMENT OF CONCOMITANT ESOTROPIA.

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WHEN I was invited by your Council, through your able Secretary, Mr. Salt, to address you on some point of interest to this Society, my first impulse was to forego the honour. I joined this Society to lessen my ignorance of optics, and felt that my proper place was among the audience, on the other hand I reflected that it behoved me as a member to obey and do what little I could to contribute to the interest of its gatherings. Hence, I appear before you to-night, and I have chosen as a subject one which I have worked at, at intervals, for a good many years, though, at first sight, it does not seem to be one quite germane to your work. I would, however, point out, in justification, that it is occasionally an advantage to follow up side issues because that must widen one's mental horizon, and in this particular case, because it indirectly affords much food for thought as to the relationship which exists between the body corporate and a simple error of refraction, and, moreover, it may assist in the proper understanding of the value of atropine in dealing with such errors. These three points will, I trust, prove a sufficient excuse for my taking up the time of the Society.

My subject, then, is the optical treatment of concomitant esotropia. I have used a term which is at present used to a very slight extent, but I have been led to do so for the following reason :—Esotropia is synonymous with convergent strabismus or squint. Before Stevens introduced his now universally adopted nomenclature for the superable squints, we had a great confusion of terms, and it appears to me neater and more logical to extend it to all aberrations of the extrinsic muscles. Thus, in abnormal tendencies to deviations, we speak of heterophoria, and in actual deviations of heterotropia (tropos—a turning). It is a self-evident proposition that before we can discuss the value of optical treat-

ment—by which I mean all treatment other than by surgical interference—we must have a clear idea of the various factors at work in the development of the morbid state.

All rays of light as met with in our natural surroundings are divergent, some greatly so when emanating from a near luminous point, others so slightly that for all practical purposes we may assume them to be parallel when the luminous point is more than 6 meters distant. In the normal eye, that is to say, in emmetropia, these parallel rays come exactly to a focus on the fovea centralis of the retina, but in the hypermetropic eye they meet beyond this point. In other words, in hypermetropia the antero posterior length of the eyeball is shorter than the focal length of the dioptric media. If a luminous point were situated in the fovea centralis, its emergent rays would still be divergent although less so than those within the eye, and entering rays following their path would be focused on the retina as in emmetropia.

But, as I have just remarked, there are no convergent rays in nature; hence, the hypermetrope must do one of two things: he must either be content with such blurred images as the diffusion circles form, or he must render them convergent. Rays may be made convergent by transmitting them through a convex lens placed in front of the eye or the refractive power of the dioptric system must be increased by accommodation. The utilisation of a portion of his total amplitude of accommodation, however, has several disadvantages, the principal of which is that he has correspondingly less for dynamic effort.

Let us take a case in point. A child of 10 has normally 14 diopters of accommodation, and can exercise sufficient dynamic refraction to focus rays from a point so near as 6 cm. from his eyes, but a child of ten who has, say, 6 diopters of hypermetropia, must utilise 6 of his total amplitude to focus parallel rays; hence he starts for all more divergent ones with an available amplitude of 8 instead of 14 diopters, which will bring his near point to 12 cm. This would appear to be sufficient, but it must be borne in mind that it would be quite impossible to sustain the effort of putting all the accommodation in force, and, even at a working distance of 15 cm., it would only be lessened by one diopter, so that

near work would be accompanied by severe headache, and congestion, and the other symptoms of accommodative asthenopia, and this condition would, of course, intensify as age diminished the power of his ciliary muscles and the elasticity of his lens.

The functions of accommodation and convergence are so intimately linked that it is difficult for anyone to accommodate for a given point without at the same time exercising a corresponding metric angle of convergence. Thus, an emmetrope, accommodating for an object at 25 cm., would exercise 4 D. of acc., and at the same time converge 4 m.a.; conversely, it is difficult, if not impossible, for him to converge 4 m.a. without also accommodating. So the hypermetropic child, who uses 6 D. of his acc. for distance, would have a tendency to use 6 m.a. of convergence, that is to say, we have more or less esophoria in every case of hypermetropia.

Esophoria is the tendency to convergence, and it is rendered latent by an equal and opposite tendency to maintain fusion; if the fusion sense is sufficiently highly educated, a high degree of esophoria may be neutralised, but if any factor should intervene which will lessen the value of binocular vision the esophoria will pass into esotropia, the tendency to converge will be translated to manifest squint. Moreover, when nearer objects are looked at and more accommodative effort is demanded, so also will the linked convergence tendency be stronger and stronger, also because in looking at a flat page the desire for stereoscopic vision is less, as only two dimensions have to be regarded.

The question which has often been asked by those who do not hold the view which I have endeavoured to map out—the view of the immortal Donders—is, "why do not all hypermetropes squint?" and we can answer that all hypermetropes do squint, but that the squint in the vast majority of hypermetropes is superable and rendered latent by the fusion sense. Further, it is only by the education of that sense that we can prevent esotropia from manifesting itself, and that we can retrace the pathological steps and lead the esotrope back to orthophoria. We see then what an enormously important factor in prevention, and in cure, is this fusion sense, and I offer no apology if I venture to enlarge a little upon its characters.

The sense of vision is exercised by two separate organs, yet the mental impression is that of a single object. It may be explained in this way: If we fix the central point of a perimeter, its image lies exactly in the centre of the fovea, and we can at the same time perceive the presence of a small white patch 20 mm. square in the periphery to the following extent.

53
47——|——95
65

If we take intermediate meridians and project the sphere on a flat surface we obtain the chart of the F.V. Now, if we place the mid point of the nose opposite the fixation spot, and chart out each eye separately, we will obtain two curves, which we can superpose and find that we have a central heart shaped area in which all objects must be seen simultaneously, and also two portions at the outer side of each field of about 35 degrees in which monocular vision is present. We are not conscious of the presence of the nose and other tissues which separate the eyes; they fuse into one and from it the vector of the combined field is projected forward as if the eye were single and lodged in the middle line; the cyclopean eye of Hering. I need not enlarge upon the value of peripheral vision, albeit it is so indistinct, as you can easily essay the experiment of fastening a pair of cardboard cylinders to your face and trying to cross at the Bank on a busy day.

When we fix an object at some distance, its image falls on each fovea and the images of the surrounding objects fall on functionally identical points of the retinæ. The higher nerve centres fuse these different pictures, and the result is a single impression. On critical examination, however, we find that we have unconscious double vision over the greater part of the visual field. The following is a quite simple experiment: Hold up the hand and close all the fingers except the first finger and the ring finger. Now fix the further one and the near one is double, and then fix the nearer one, when the far one is double.

Not only so, but repeat the experiment this time, slowly closing the eye so that its image gradually fades, and you will not have much difficulty in coming to the

conclusion that when the far one is fixed and the left eye closed the R. side disappears, while if the near one is fixed it is the L. hand image that goes, *i.e.*, we have in the one case crossed diplopia from relative divergence, and in the other homonymous diplopia from relative convergence.

This physiological diplopia is constantly present, although the mind is unconscious of its existence. Still some of the lower nerve centres not only are conscious of it, but analyse it inasmuch as we are instantly able to tell whether divergence or convergence are called for without having to consciously say whether the diplopia is crossed or homonymous, without, in fact, being conscious so far as thought is concerned of the existence of diplopia at all. It is this sub-conscious reflex which we know as the faculty of binocular vision or the fusion faculty which gives us the impression of the third dimension in space. Most people never saw physiological diplopia until the fact was pointed out.

The elasticity of the fusion sense is well seen in the case of prisms. A prism, as you all know, deflects a ray of light towards its base. On looking with one eye at an object, that object is projected towards its apex. If now we place a prism, base in, in front of one eye, and the other be left open and the gaze directed to a given point of fixation, two images are seen, a true one seen by the other eye and a false one seen through the prism. The eye at once makes an outward deviation, *i.e.*, towards the apex, so as to throw the image on its macula when the object is seen singly. Let us use, instead of the ordinary prism, the Herschell or double rotating prism and gradually increase its strength.

A point is soon reached, the outward breadth of fusion, beyond which single vision cannot be maintained. This does not indicate that the images are no longer formed on identical retinal areas, for these have been passed, and, indeed, that is the keystone of the proper understanding of the fusion sense. It is more than simple binocular vision, the result of the stimulation of identical retinal areas, it is the psychological fusion of separate impressions, and, to evidence that, a further experiment is necessary.

Let us take two tubes, *e.g.*, those of P.S. heteroscopes, and in the end of each place a luminous line such as

this ||. At the outer side of the lines two small holes are placed, one coloured green and the other red. These are control marks to show that the image is not being suppressed by one eye. Now gradually separate the ends of the tubes. At the commencement, the pictures are seen thus, $\begin{matrix} R & G \\ \cdot & \cdot \\ o & o \end{matrix}$ but as the limit of the fusion breadth is reached the control marks gradually separate thus, $\begin{matrix} R & G \\ \cdot & \cdot \\ o & o \end{matrix}$. We know that they cannot really have moved in the apparatus, and so we must conclude that they have been formed on retinal areas which are not identical, but the line remained single in spite of that by the fusion elasticity of the higher cerebral centres.

One may distinguish three grades of binocular vision:—

1. The lowest in which both eyes are capable of receiving visual impressions and transmitting them simultaneously to the brain, but, in these, the fusion sense is absent. We see such a condition in diplopia due to paralysis, or in concomitant esotropia where the image of the deviating eye has been made perceptible by the presence in front of it of a coloured glass.

2. Simultaneous vision with fusion. In this class the fusion sense is present, but is relatively weak, and after a time is discarded in favour of monocular vision.

3. Simultaneous vision with a strong fusion sense and stereoscopic vision. This is the condition normally present. As soon as diplopia is evoked, the eyes immediately strive to overcome it, and failure to do so causes distress. The stereoscopic sense is also present, and the eyes, seeing a solid object from different points of view, not only fuse the impressions, but out of them construct the psychical sensation of solidity. We may put them thus: Elementary class, simultaneous vision; more advanced class, simultaneous c. fusion; educated class, simultaneous c. fusion and the power of estimating the third dimension.

To say whether simultaneous vision is present or absent, we can employ several tests.

1. The simplest is Stellwag von Carion's. In this, the patient gazes at an illuminated sheet of white paper. We place in front of one eye a red glass and the white

surface seems to be suffused with a pink tint. We transfer the disc to the other eye and the same thing occurs.

2. Snellen's Coloured Type.—The patient is armed with a frame bearing two coloured glasses—one red and one signal green. His attention is then directed to the transparent coloured letters, which, when seen simultaneously, form the word "friend." If he does this, he has binocular vision, but if only one eye is being used he reads either "fin" or "red, according to which one is used.

3. Javal's Bar Test.—In this, a cylinder, such as a lead pencil, is held before a page of print at a distance of about two inches. If only one eye is used, the patient stops reading when he comes to that part covered by the pencil, but if both eyes are in use, he reads the whole line easily.

4. Graefe's Test.—A prism, base out, is placed before one eye; if the patient rotates one eye so as to fuse the double image caused by the prism he has binocular vision.

5. Hering's Drop Test.—This is a curious test, in that it tests the psychical sensation rather than the muscular sense. It consists of an oblong box bearing two arms at its distal end between which a bead is suspended. This is the object of fixation. Small objects, such as peas or shells, are dropped in front or behind the bead and a person with normal vision can at once say whether it was dropped in front or behind. The portion of time during which the falling body is in the narrow field of vision is too short to permit of any muscular movement of acc. or conv. It is really a test for the sub-conscious physiological diplopia, of which I have just spoken.

Simple binocular vision is present at birth, but the fusion sense is not formed until it is at least six months old. When the infant is about a year old, we have evidence of complete binocular vision.

I will now return to a more detailed description of the causes at work in the production of concomitant esotropia. The eyeball is moved in its various directions by the cone of muscles occupying the hollow cone of the orbit. There are four straight muscles or recti, and two muscles acting transversely, the superior and inferior obliques. The action of the obliques, in conjunc-

tion with the superior and inferior recti, is very complicated, and, although of the greatest interest and beauty, does not immediately concern us.

What is important, however, is to bear in mind that the inward and outward rotations take place about a vertical axis lying in Listing's plane, and that the internal rectus rotates the globe inwards, while its opponent, the external rectus, moves the globe outwards. Further, you must bear in mind Hering's law of equal innervations. This law is to the effect that the one eye cannot move in any direction without an identical nerve impulse being sent to its yoke fellow of the other eye so as to cause an exactly equal and parallel movement.

We cannot, *e.g.*, turn one eye up and another down, or both outwards, and the reason of the presence of this conjugate reflex is the necessity of preventing double images, which would serve no useful purpose, and which, necessarily, must occur if such independent action were permissible.

The internal and external recti, then, are those which play the greatest part in esotropia. The internal rectus receives its stimulation along the fibres of the third cranial nerve, while the externus has a special cranial nerve all to itself—the abduceus or sixth nerve.

The nucleus of the third cranial nerve lies in the hind part of the floor of the third ventricle, and in the front part of the ventral surface of the aqueduct of sylvius opposite the superior quadrigeminal body. The cells of the anterior part are smaller than the others, and it is now established that the nucleus is a colony of more specialised ones. The researches of Hensen and Völckers, of Starr and of Nussbaum—who experimented on kittens—have shown that the anterior portion acts specially on the ciliary muscle, the lateral ones on the sphincter of the pupil, and the posterior ones to the recti muscles.

The nucleus of the sixth cranial nerve is contained in the dorsal surface of the pons cerebri, close to the floor of the fourth ventricle and immediately above the striæ acousticæ, and may be regarded as containing the cells governing the divergence reflex.

By means of a bundle of fibres from the mesial part of the posterior longitudinal bundle, the nucleus of the sixth is joined to that of the third on the opposite side.

In the diagram one set of efferent fibres from the sensorium are shown uniting the lateral nuclei of the sixth with the opposite cortical areas, and serve to indicate the path of conjugate deviation stimuli.

The lenticular or ophthalmic ganglion is a centre for the supply of motor, sensory, and sympathetic nerves to the eyeball. The posterior border has three roots, the motor one from the third (branch to the inferior oblique), the long or sensory one (nasal branch of ophthalmic division of V), and the middle or sympathetic (cavernous plexus). From the front surface, six or eight short ciliary nerves are given off which divide as they go forwards, so that they form from 12 to 20 filaments as they reach the eyeball. They pierce the sclerotic, and, running in grooves on its inner surface, are distributed to the iris, the ciliary muscle and the cornea.

[The lecturer then gave a short description, illustrated by lantern slides, of nuclei: the kinesodic system, slight hypermetropia, hypermetropia calling on convergence centre, hypermetropia with convergence tendency, neutralised by fusion sense (esophoria); esotropia—effort exceeds pos. part of rel. acc. and binocular conv. takes place; esotropia—one eye rotates into fixation, other subtends doubled angle.]

In this we render an emmetrope artificially hypermetropic by placing a concave lens in front of the eye. We will suppose that the observer who is conducting the experiment to be emmetropic, and that his retinæ are equal in visual sensitiveness. Let him commence by placing a − 2 D. before each eye. He will still see 6/6 by using 2 D. of his accommodation. Now let him gradually increase the cc. lenses and a point will be reached, say 5 D., which is the highest lens he can overcome and preserve single vision—this, of course, is the measure of his relative accommodation for that point.

Let him now close one eye and ascertain the highest cc. lens with which 6/6 is read; he will probably overcome about 8 D., and this represents the absolute accommodation of each eye singly. Let him now place 5 D. lens before each eye, and while viewing 6/6 singly, place in front of one eye a red glass. Homonymous diplopia at once appears—now, why?

Because you have, by using the coloured glass, sensibly diminished the visual acuity of that eye, and so lowered the tone of the fusion reflex; the sixth nerve no longer conducts equal and opposite impressions, and the esophoria which was present throughout becomes manifested as esotropia.

To make the mechanism, if possible, still more obvious, I have constructed this model, and I will endeavour to construct on it the points illustrated by the lantern. [Here the speaker showed a large model in which the various movements of convergence, divergence, and conjugate lateral deviation could be reproduced, by a system of weights and cords. By its aid he showed the overflow of nerve impulses to the convergence centre, being neutralised by stimulation of the divergence centre, resulting from the desire for fusion. He also showed the binocular nature of esotropia, and the test for concomitancy.] Strong as these physiological and experimental evidences seem to me to be, I propose to call a yet more conclusive and practical witness, namely, the results obtained by clinical investigations among those really affected. If my conclusions are correct, we ought to find that esotropia only occurs amongst hypermetropes, whose fusion sense has become diminished from some ascertainable cause.

Well, in the first instance, I will draw your attention to this table or chart which shows the result of examining the refractive condition of 1,000 consecutive cases of hypermetropia, varying in extent between one and ten diopters. Where astigmatism was present, the amount was taken as the means of the two meridians. We find that the commonest error was between 3 and 5 D. To particularise more minutely: 56 were between 0 and 2 D., 124 between 2 and 3 D., 223 between 3 and 4 D., 230 between 4 and 5 D., 168 between 5 and 6 D., 82 between 6 and 7 D., 67 between 7 and 8 D., and 50 between 8 and 10 D.

Of those who did not have esotropia, there were 49 between 1 and 2 D., 112 between 2 and 3 D., 203 between 3 and 4 D., 204 between 4 and 5 D., 127 between 5 and 6 D., 69 between 6 and 7 D., 64, between 7 and 8 D., 48 between 8 and 10 D.

Of the balance of 123 who were esotropes = 8 per cent. —and this figure is the only one I have yet met with

which gives a percentage founded on clinical investigation—7 were 1.2 D., 12 were 2.3 D., 20 were 3.4 D., 26 were 4.5 D., 41 were 5.6 D., 13 were 6.7 D., 3 were 7.8 D., 2 were 8.10 D.

We find the commonest refractive error between 3 and 6 D., and what does that show? It shows that it is in those cases where the greatest call on the acc. is made, that squint is commonest. If the refractive error is small, the esophoria is the more readily rendered latent; if the refractive error is high, no acc. effort can overcome it, and the individual rests content with deficient vision. I collected statistics, however, of 510 more esotropes, and adding them to the above 123, I show you next in graphic form the fluctuation of the refractive error in this very large number. Here again we see the same curve. Below 1 D. only 11, below 2 D. only 66, below 3 D. 130, below 4 D. 131, below 5 D. 191, below 6 D. 56, below 7 D. only 18, below 8 D. only 15, below 9 D. only 3, below 10 D. only 2.

The same rarity in the low degrees where the fusion is proportionately high, and the high degrees where convergence can do so little, and the same great frequency in the medium degrees where the struggle is strongest between convergence and binocular fusion.

But, it may be objected, if the contention that esotropia is always due to increased innervation of the convergence centres with diminished fusion stimulus to the divergence is to hold good, how can you explain the presence of emmetropia and of myopia in these cases? I show you here a chart incorporating the examination of 650 esotropes, of whom 633, or 97.4 per cent., were hyperopic, and the remaining 2.6 per cent. were emmetropic or myopic. To begin with, notice the overwhelming preponderance of H. The emmetropes are easily accounted for; their average age was 18 against the average age of hyperopes, which was between 5 and 6 years.

Note that this is the average age when they came under observation, not the average age of the onset of esotropia. They were then, generally speaking, three times as old as the hyperopes. Now, the researches of Königstein, of Ely, and of Horstman, of Schleich, and of many others have shown that the refraction of the normal new born eye is hyperopic to a considerable extent.

We know also that that of the young adult is emmetropia; hence, it is certain that the infant eye undergoes an antero posterior lengthening.

Now, if this is so—if H. of 2 D. fades into E., is it not equally clear that H. of 3 D. may become E. at an age of 18 years? There is reason to believe, too, that just as many children take longer than others to co-ordinate the extrinsic ocular muscles, so they might vary in their power to control the intra-ocular ones. Given any paresis of the ciliary muscle in presence of low H., it is clear that a greater effort would be needed to induce it to act, and *pari passu*, a greater effort of convergence would be associated with it. In other words a hyperopic infant, with an unresponsive ciliary muscle is far more liable to develope esotropia than one with an equal error —if error it can be called, and a normal muscle. My view of emmetropia is this, then, that in consequence of the axial development of the eye the H. lessened to E., but the faulty innervation with its badly educated fusion sense have become habitual and permanent.

Next, as to the myopes. Of these there were but six, and here again the ages were significant, for they were all over 18. The amount, too, was significant, in none was it over 3 D. Now, we know that while H. is congenital, M. is acquired. To my mind it is clear that the H. had dwindled to E., and the axial lengthening had gone on to low M. So, then, there is nothing in these other refractive conditions incompatible with the theory I have advanced.

The last point in causation which I will raise is the age of onset. I have carefully enquired into this, and it, like the foregoing observations, coincides with the experimental evidence: 27 commenced during the 1st year, 92 during 2nd, 148 during 3rd, 205 during 4th, 113 during 5th, 31 during 6th, 18 during 7th, 12 during 8th, 13 during 9th, and 1 during 10th; *i.e.*, 466 of 650 commenced between the 3 and 5 years of age, or 71 per cent.

Nearly three-quarters, that is to say, commenced between 3 and 5 years of age. Now, what does a child do about this age? It directs its close attention to near objects. Its metric angle of convergence is greater and its accommodative effort is greater than the normal adults, because of its short arms; and these functions,

when looking at picture books and dissecting toys, is nearly always in action during its waking hours. It is then during that period when the acc. and the conv. is most called into play that we find the onset of esotropia commonest.

We have other views of the causation of esotropia. Thus, we have the popular view of imitation. One never meets with a case in which the parent has not a long story to tell of imitation, or fits, or other theory, told with a minuteness of detail which "suggests the tale oft told." As they are ignorant of ametropia, it would be fruitless to look to them for the real reason.

Then we have the myopathic school, led by Graefe, Schweigger, Segger, and Schell, who look for the cause in a muscular anomaly. If the muscle is really shortened, then we must have several conditions.

1. The extent of outward rotation of the eye must be diminished to the extent of the shortening. This it never is. The esotrope can abduct the eye to the same extent as the hyperope.

2. The shortening, if structural, must be permanent. This, again, it never is. During sleep or under chloroform, it quite disappears. This not only disproves the myopathic view, but it is additional proof of the neuropathic one.

In practice we can distinguish three grades of esotropia:—

1. Periodic.—In this the esotropia is only occasionally manifested. The great strain on the convergence centre is balanced by an equally strong fusion reflex, but if from any cause the general physical tone of the individual is lessened, we have the opposing and higher reflex lessened to, and esotropia shows itself. So that we find the child squints only when it is ill or run down, during and after a holiday the eyes are all right. This is just the class of case where optical treatment gives the best result; if neglected, it goes on to the next or the final type.

2. Constant Alternating Esotropia.—In this class the esotropia is always present, but it may manifest itself in either eye. Both eyes have an equal visual acuity, but the fusion sense is only rudimentary; hence, monocular vision, assisted by convergence, is preferred to the slightly felt advantage of binocular vision.

3. The Fixed Esotropias.—In this, the largest class of case, we find some factor which lessens the visual value of one eye, and, therefore, the better one is always used in fixation. These factors may be quite slight; thus, half a diopter of astigmatism is sufficient. Generally speaking, any opacity of the dioptric media, whether in the cornea or lens, produces it. Congenital amblyopia, in which one retina is deficient from birth, must also cause it.

The one great intervening factor, however, which determines the transition of esophoria into esotropia, is weakness of the fusion reflex; temporarily weakened in periodic cases, permanently in abeyance in the alternating and the fixed.

In esotropia, the visual axes do not intersect at a point of fixation situated in the median plane; only one eye fixes, the other is rotated into a doubled metric angle. Now, when this is the case one of two things must happen; either the individual must have homonymous diplopia or he must mentally suppress the false image formed in the deviating eye. The images are homonymous because the retina always projects the image along the path of entering rays.

This suppression is rendered easier by the lowered sensibility of that portion of the retina on which the eccentric image falls. In illustrating the relative acuity in the form of a pyramid with its apex on the macula you will see how the relative luminosity makes suppression easy. It may be thought that the deviating eye would be conscious of the image in a peripheral part of the field lying in its visual axis, but this is always suppresses, even in paralytic heterotropia, where the false image cannot be suppressed. The patient does not see two objects, but two images of the same object.

In suppression amblyopia, it is often possible to evoke diplopia by placing in front of the fixing eye a red glass and using a candle as a fixation object. The deviating eye may then become cognisant of the more brilliant image. In the position taken up, the nasal half of the retina suffers less than the temporal side, which is practically never used. There are, no doubt, two distinct varieties of amblyopia—congenital amblyopia and amblyopia exanopsiâ, or suppression amblyopia. We can distinguish congenital amblyopia to some extent by

its intensity. Such eyes are almost blind, vision being reduced to fingers at a few feet, and never being more than 6/60.

Another test is for the relative perceptivity of the nasal half. Place a shade over the fixing eye, then, when the patient looks straight forwards at the deviating eye, move a white object, such as a visiting card, about in front of the nasal part of the field, so that its image is formed on the temporal half of the retina. If it is seen, reduce its area until invisible; now do the same on the other side, and the object, previously invisible, will be clearly seen.

It is owing to this fact that in suppression amblyopia the nasal portion is relatively acute, that we meet occasionally with the variety termed strabismus paradoxus, in which it has been supposed fresh areas of physiological identity have been developed; such an eye, on being directed to fix an object, instead of rotating the axis outwards rotates it further inwards so that the image falls on a more exercised portion. As a matter of fact, new identical areas are not developed, as true binocular vision is always absent.

Suppression amblyopia differs from congenital amblyopia in the above manner, and also by its less intensity. One can often raise vision in that eye from 6/60 to 6/18, or thereabouts, and by its curability. That is to say, we can reverse the process of amblyopia and train the retina back to its perceptivity. We see such a condition in people with straight eyes, hyperopes and astigmats, who have never seen clear images. In these, vision improves by correction to a subnormal degree, but after wearing glasses for some months or years it becomes normal. This is amblyopia from defective retinal images, and is a degeneration of function exactly akin to suppression amblyopia. If the eye, however, is not properly exercised, it loses macular supremacy and becomes very blind, so that when directed to fix it makes slow nystagmus-like movements in its efforts to find an area of superior perceptivity.

Diagnosis.—It is always an easy matter to decide the presence of heterotropia in severe cases, but in slight cases it is by no means easy; however, there are several tests at one's disposal, the most familiar of which is the "cover test," first described by Elliottson, of Carlisle,

in 1840. Standing in front of the patient, we direct his attention to the bridge of the nose, and while he fixes it, bring gently in front of the eye a card or disc. The uncovered eye at once makes an outward movement to fix. If no movement takes place we occlude the other eye.

Having decided which is the fixing eye we mark on the lower lid a spot of ink just below the pupil centre of each eye. Then covering the eye, we mark its centre when converging, and the centre of the eye now fixing. The interspace indicates the primary and secondary deviations.

The primary and secondary deviations must be carefully noted, because of a point of great importance. In concomitant esotropia, the primary and secondary deviations are equal, while in esotropia, due to paralysis of the sixth cranial nerve, the secondary is greater.

A further distinguishing feature is the diplopia, which, in paralysis, is always dominant and distressing, and is the ground on which the patient seeks advice; whereas in concomitant esotropia, the diplopia is absent, and the patient comes on account of the deformity.

Javal uses a ground glass screen instead of an opaque disc; this has the advantage of enabling us to see the eye deviate while it cuts off the patient's vision, and causes him to fix with the other. In a case of low degree not only have we to eliminate paralysis, but we have to say whether esotropia is really present, for in some cases it may be simulated only.

In some cases of myopia, the eyes have an appearance of convergence which a careless observer might readily mistake for true esotropia and which probably has assisted in the view that myopia is not incompatible with concomitant esotropia. The diagnosis of these cases is an essential part of our work in dealing with esotropia, and to make it clearly understood, permit me to draw your attention to the various antero posterior axes of the eyeball. To begin with, we have the geometric axis. That is the line which joins the mathematical centres of the anterior and posterior poles.

It is often spoken of as the optical axis, because it closely coincides with the optical centres of the dioptric media, but it does not exactly do so. The name optic axis is a frequent source of confusion to students, who,

not unnaturally, assume that the optic axis indicates the path of rays from a luminous point of fixation. Hence, I propose to discard the term optic axis, and substitute that of *polar axis* as being more definite.

Donders assumed that this polar axis was coincident with the major axis of the corneal ellipsoid, but that is now disproved. Besides the polar axis, we have two others, the visual axis and the fixation line. The visual axis indicates the path of rays from a point of fixation through the nodal points in the lens, and onwards to the centre of the fovea centralis. This axis traverses the cornea to the nasal or inner side of the polar axis. The third line, or fixation line, is that joining the point of fixation with the centre of rotation of the eyeball. We have, therefore, several angles formed by the intersection of these secondary axes, with the polar axis. Thus, we have the *angle alpha, of Landolt*. This is the angle between the polar axis and the major axis of the corneal ellipsoid.

Then we have the *angle alpha of Donders*, formed by the intersection of the major axis of the corneal ellipsoid with the visual axis. Donder's angle is now discarded, and Landolt's does not immediately concern us. So that when we speak of the *angle alpha*, we mean the angle formed by the intersection of the polar and visual axes, in other words, the sum of the angles of Donders and Landolt.

Lastly, we have the *angle gamma*, which is formed by the intersection at the centre of rotation of the eyeball of the polar axis and the visual line, and it is this angle which we measure by corneal images. However, it is so nearly identical with alpha that the observations of one may be considered as applicable to the other.

The angle was first observed by Helmholtz, but it was Donders who showed its variations in ametropia. Thus, in emmetropia, the average is 5 degrees, in myopia 3 degrees, and in hypermetropia 7 degrees. I was interested to find that it was noticed and explained by Porterfield, of Edinburgh, so long ago as 1759, in the following words: "The Strabismus may proceed from a Fault in the first Conformation, by which the most delicate and sensible Part of the Retina is removed from its natural Situation, which is directly opposite the Pupil and is placed a little to aside of the Axis of the

Eye, which obliges them to turn away the Eye from the Object they would view, that its Picture may fall on this most sensible Part of the Organ."

Well, as I have said, in emmetropia the angle is about 5 degrees, so that we look obliquely out of our eyes. In hypermetropia the angle is increased, and when greatly so the eyes appear divergent. Conversely in myopia, it is less, and may even be coincident with the polar axis or go beyond it and traverse the cornea to the temporal side, in which it is spoken of as negative and gamma, and this gives the effect of convergence which I have already mentioned. There are two ways of investigating the angle γ.

In the first, we cause the eyes to fix an object in the middle line at a distance of about one metre, and then gradually approach it to the punctum proximum. If the eyes steadily follow it until that point is reached, and binocular fixation is maintained throughout, the case is one of apparent and not of real esotropia.

Secondly, we may observe the corneal image. The cornea acts as a small convex mirror of 4 mm. focus, in which we see the tiny erect image of the luminous point. Many observers use the ophthalmoscopic mirror flashed into each eye alternately, but, personally, I prefer a small incandescent lamp held just below the observer's eye, for this permits the images to be seen simultaneously, and any movement of concomitancy is readily noted. The patient must fix the point of light, otherwise the images are untrustworthy. To estimate its amount, I use the Priestly-Smith tape, noting the position of the corneal reflex and then moving the hand outwards until the reflex is exactly central. One can, as Maddox says, guess the probable amount of ametropia by the size of gamma—if low, suspect myopia; if high, hypermetropia.

There are many ways of measuring the amount of esotropia. A fairly accurate guess may be made by noting the position of the corneal reflex, as shown by Hirschberg. If it lies midway between the centre of the pupil and the edge of the pupil, it is about 10 degrees. If on the edge of the pupil 15 degrees. If half way between the edge of the pupil and the edge of the cornea 25 degrees. If on the edge of the cornea 45 degrees, and if midway between the corneal edge and the outer canthus 60 degrees.

The method I generally employ is that of Priestly-Smith, of which there are several imitations, but none so good as the original one. A tape one metre long is provided with a ring at each end. The patient holds one ring against his temple, the operator holds the other end with the mirror; a second tape, graduated in tangents to degrees, or metre-angles, slides through the fingers of his other hand, the movement of which is followed by the better eye. As soon as the corneal reflex of the deviating eye is central the measure is read off.

Maddox's method is identical with this. Instead of following the hand the eye reads figures denoting tangents to metre-angles until the observer finds the central corneal reflex.

Measurement by the perimeter, introduced by Javal, is still in vogue, but is open to several objections. To begin with, the operator's head gets in the way, and it is consequently impossible to measure small angles, and, besides, the apparatus is clumsy and difficult to adjust.

Charpentier tried to get over the difficulty of small angles by placing a candle at the point of fixation, and noting the point on the arc where the reflex was central —the axis of the eye would be half the angle noted, because L. of incidence and reflexion are equal. It, in its turn, is open to the objection that the light reflex is projected on the caustic curve of the cornea, because of its spherical aberration.

The old linear methods have quite gone out of use; one, however, meets with these curiosities. The most popular was that of Sir William Lawrence, a curved ivory plate marked in 1/12 of an inch, which was applied to the lower lid of the deviating eye. The zero of the scale was central, and corresponded with the centre of the pupil if the eye was straight. The adversion of the eye was then noted by reading off the position of the pupil centre. I repeat that in the measurement of esotropia no method is so simple and accurate as that of Priestly-Smith.

To measure the vision of adult esotropes is quite a simple matter. We simply place the patient in front of Snellen's type and ascertain the acuity for each eye. I would, however, suggest the desirability of really closing the better eye when the esotropic one is being

tested. I never now trust the trial frame, but exclude the eye with an eye shade. I once came an awkward cropper on this very point. A medical man, who was in the habit of referring a good many cases to me, sent his son who had esotropia for treatment. One eye was amblyopic and I directed orthoptic treatment. A year after he came and I wrote a glowing letter to his father, stating that vision had improved far beyond my anticipations. After six months he came again with a note to the effect that the vision seemed unsatisfactory.

I used the eye shade and was horrified to find that the vision was exactly where it was when I first saw him. Obviously, he had on the second occasion looked past the occluding disc. Of course, I had to explain that the good result was a mistake on my part, but it was a serious mistake for me, as since then I have had no more cases from that source. I therefore would advise you to exercise great care in these cases lest a like experience should befall you.

Worth has introduced a method for testing children too young to read, by means of a series of ivory balls, varying in diameter from $\frac{1}{2}$ in. to $1\frac{1}{2}$ ins. These are rolled on the floor at a distance of six feet. The little patient wearing a shade over the better eye runs after these, and, thus, by the size of the ball seen, some relative estimate can be made. I have been unable to regard them, however, as in any way the equivalent of Snellen's type.

You all know that a patient who can only read 6/18, *e.g.*, could say how many letters there were in the 6/9 line, although he could not read any of them. The whole letter subtends an angle of 5′ at the eye at the distance they should be read, but the individual lines of the letter subtend only 1′. So in the 6/9 line, the 6/18 patient can see the 5′, but not the 1′.

Hence, a child, who could see an ivory ball equalling a 6/9 letter at 6 metres, could not be regarded as having an acuity of 6/9. Again, an object in movement is far more readily distinguished than one at rest, *e.g.*, in scouting, the horses are picketed with the heads towards the enemy's lines, so that the waving of the tail may not be seen, as it is much easier to see the moving tail than the still horse.

I have endeavoured, in this work of art, to arrive at

something like an approximation to Snellen's type. It represents an object familiar to little ones, but which, I fear, requires some explanation to you. It is a figure of Mr. Punch, so dear to all infants, and that figure can only be recognised as his by the head, which, at 60, subtends 5 m., being equal to 6/60, and its lines subtending the same angle as Snellen's type. The same holds good of the other points, thus, the baby's face = 6/36, the bottle 6/24, the three legs 6/18, the crown 6/12, the hat 6/9, and the moon 6/6; all the lines are exactly the same thickness as Snellen's.

I place this picture at 6 metres and say, Who is that? If successfully answered, I ask questions in order: What is he carrying? What has the baby got in its hand? How many legs has Mr. Punch? What has he on his head? What is on the baby's head? Where is the moon? Thus, I get a comparative result. If he cannot be distinguished I bring him nearer until he is, then, in recording, I say Mr. Punch to crown = 6/12.

The keynote of treatment is this:—We must reverse the processes by which esotropia has manifested itself, and, in optical treatment, we have four separate means at our command, all of which have to be used.

1. Correction of the Hypermetropia.—This is the most important measure. The correction must be very exact. The process I always adopt is, first, to register the visual acuity by the Snellen distance type, or, if the patient is too young, by the "Punch" test. Next, I order atropine to be placed in each conjunctival sac of a strength of four grains to the ounce, four times daily for four days. It is useless to order the sulphate, the pure neutral alkaloid must be used, as this alone is soluble in vaseline.

A small portion, the size of an oat seed, is inserted on the point of a conical glass rod. The ointment has many advantages over the aqueous solution. The disadvantages of the latter are, its liability to decompose, and the chances of absorption by some running down the cheek into the mouth, which a child always has open when anything is being done to its eye.

When the patient returns, the pupils are widely dilated and accommodation is wholly suspended so that the total hyperopia is unmasked. No one who is not in the habit of constantly using it can have any conception of the

difference between given cases before and after its use. At the preliminary skiascopy, the shadows may reverse with, say, a + 2.0 D. sp., and after atropine we may find 6 or 7 D.

The disadvantage of atropine is that it exposes to the light reflex the basilar portion of the cornea, the reflex of which often travels in a direction opposite to the central reflex. However, with practice, one soon becomes accustomed to neglect the former and concentrate the attention on the movements of the tiny central reflex. I invariably use the plane mirror at a distance of one metre, while the source of light is shielded and very small—only 5 to 7 mm. in diameter, *i.e.*, about the size of a dilated pupil. Thorington's chimney, with an iris diaphragm, is the ideal illuminant.

Having worked out the error with the greatest care, the glasses are ordered for constant wear, and until these have been obtained, the atropine is ordered to be continued twice daily. When the glasses are in situ, the atropine is stopped. The reason is that the ciliary muscle has been accustomed to act excessively, and that powerful innervation has become a habit often quite difficult to break. The child puts its accommodation in force for distance, and so renders itself artificially myopic. With atropine, this occurs to a far less degree. The ciliary muscle gradually recovers its tone after the long rest, and settles down when the foveal image is at its best.

I attach the greatest importance to the accurate fitting of the glasses. Whenever possible, I order periscopic ones, as they are optically better and less liable to get smeared by the lashes. The lenses are invariably circular, and should be as large as possible, so that the child cannot look past the edge. In astigmatism it is difficult to obtain the periscopic effect, although it is occasionally possible in cases of high astigmatism, by placing the concave cylinder next the eye. Opticians would confer a vast boon on the community by rendering it possible to obtain toric lenses at a reasonable price.

I never now order glasses under two years of age. Not because the child, in falling, is liable to injure his eyes with fragments, for the lenses simply crack across, but because they never remain clean for any time. I

have often tried glasses in children under two, but have never been able to obtain any benefit to counterbalance the drawbacks. After two, however, I do not hesitate to prescribe them. As I have said, the lenses should be circular and the bridge a wide W.

The ear pieces should be short and looped so that they can be tied in position—curl sides should not be used under four years of age. When the glasses have been obtained, if they have a cylindrical element, I always test them to see if they can be made to rotate in the frames, and if they do I send them back to be cemented in.

Now, what is the value of correction in treatment? Lang and Barrett, who investigated this point, found 10 per cent. permanently cured and 33 per cent. cured while wearing the glasses, or 43 per cent. I have not been able to go through all my results, but, in 1896, I had records of 285 I had treated out of 322 cases of esotropia. Of these, 38 permanently cured or 13.3 per cent., 114 cured while wearing glasses or 39.9 per cent., 18 improved, but still converging, or 6.3 per cent., *i.e.*, 59.5 per cent., or, not counting improvements, equals 53.2 per cent.

This is a higher figure than most investigators seem to arrive at. Thus, Worth gives only one-third of all cases. Of course, various operators vary in technique; however, I worked out all my cases myself, and am, therefore, pretty confident of their accuracy, and have no hesitation in saying that over one-half can be cured by glasses. Of course, the patients cannot do without the glasses unless their hyperopia diminishes; if it proceeds to practical emmetropia, they can dispense with them, but if the refractive error remains, it obviously needs glasses.

2. Continuous Occlusion.—The covering of the better eye was originally advocated by Beer, in 1800, although I was much struck with the observation of Buffon, in his "dissertation sur la cause du strabisme ou des yeux louches," presented to the French Academy in 1743, that the fixing eye should be covered to accustom the deviating eye to fix and project properly. The mistake made was that this covering was only carried out for a short time daily—about two hours.

Now, Javal has shown that we cannot obtain any im-

provement if both eyes are allowed to act simultaneously. My method, now, is to cut the lashes close to the lid margin, and fix them together with a vertical band of adhesive plaster. Over this I fill up the orbital hollow flush with layers of gauze, and, finally, cement it down with collodion. This pad remains for ten days. When the patient comes again, the vision is tested, the deviating eye covered and the pad removed to permit of disinfection of the eyelids, and then it is re-applied.

The improvement of vision of the amblyopic eye is often startling. In two weeks it may run up from fingers at 6 feet to 6/36 sn., or even 6/18. I continue this treatment for a month, at the end of which time the retina has recovered as much acuity as it is likely to do.

My next step is to train binocular vision. In special cases, I order the lens for the better eye to be ground in smoked glass to equalise the luminosity of the image. That, I am convinced, is an important point in treatment.

Having obtained the lowest grade of binocular vision we have to raise it to the second grade by educating the fusion sense, and in this connection I would draw your attention to a method very little used, but which has, in my opinion, much to commend it—I allude to prism treatment. We place a candle at 4 metres and then find the weakest prism, base out, which fuses. This prism is divided between the eyes and the combination, with spheres ordered for constant wear for a month.

Next time we again measure the fusion and prescribe weaker prisms, and so we proceed until the fusion is perfect. I have not tried this method to any extent, because of the great expense involved, but if here again this could be reduced I should be only too glad to use it more extensively, as I am sure it is a physiologically correct method.

The best stereoscope for enabling fusion exercises to be carried out, is the so-called amblyoscope of Worth. The child is placed between the operator's knees, and incomplete pictures are placed in the tubes; he is shown how to fuse these, etc. As I have said, binocular fusion commences in infancy, about the sixth month, and it would appear to attain its full development about five years of age, so that the age of patients suitable for

fusion training is from three to five years. After six the sense is permanently lost, and true binocular vision is lost with it. When fusion can be maintained, and the eyes are fairly equal in visual value, reading should be carried out by the "bar" which has been already described as a test for binocular vision.

In infants too young to wear glasses, the better eye must be kept continuously under atropine. You have seen that the whole train of symptoms commences in the ciliary muscle of the fixing eye. If we paralyse it, the pathological process comes to a standstill. One often sees an esotropia completely disappear so long as the eye is kept atropised. The infant soon learns to regard near objects with the unatropised eye, and distant ones with the atropised eye. In this way we set up an alternating instead of a fixed esotropia, and, as in alternating cases, we find the retina has not suffered from disuse.

In cases where binocular vision has been lost by opacities in the media, or by congenital amblyopia, we cannot, of course, obtain any improvement other than by operation. As these cases, generally speaking, constitute one-third of the total number, we are entitled to conclude that two-thirds of all cases of esotropia are curable without operation, provided they are placed under treatment early enough. This is the greatest point of all; you can never send the case too soon, but you can easily defer doing so till too late.

Will you forgive me, if, in conclusion, I point a moral? Until quite recently, I enjoyed the comfort of obscurity, but a few months ago I was led to enter the arena of optical optics, by my sympathy with a movement for greater legitimate freedom, inaugurated by a circle of well educated and reasonable Opticians. I would earnestly recommend scientific moderation, which is liable to be exceeded in the heat of debate, or when smarting under injustice. The refraction of the young eye is a point in which I have had a daily experience, ranging over a good many years—over twelve years—and the more I see of it,, the more convinced I am that absolutely trustworthy results can only be obtained by atropine.

With the recent demand, therefore, that school children should be sent to the Optician for treatment, I have

no sympathy whatever. Nor can I regard the statement that atropine can be dispensed with in refraction work as other than a foolish tilting at a windmill. In the adult eye, the case is entirely different; atropine is rarely necessary, and if the Optician made a point of referring to the oculist all cases in which a satisfactory visual acuity is not arrived at, I do not see what possible objection there can be to leaving the correction of visual errors in his hands, provided that he has undergone an accepted course of study, or has proved himself capable by examination.

In thanking Dr. Ettles, Dr. Drysdale hoped that he would be able to be present at the meeting on November 30, when a discussion on the paper would be held.

Discussion.

Mr. Dixey, in opening the postponed discussion on Dr. Ettles' lecture, stated that although he was not present at the lecture, he had read Dr. Ettles' paper in The Optician and Photographic Trades Review, and he congratulated Dr. Ettles for having given so interesting a paper, and also the Society for having had the privilege of listening to it. There were one or two things, however, he would like to say. First of all, there was one very useful observation which he himself had not noted before, and that was, that in looking at a printed page, the fusion sense loses the stimulus of the third dimension. That, he regarded, as a useful thing to know, and one which it was well to keep in mind. Another very interesting point, to the speaker's mind, was when Dr. Ettles, speaking about the P.S. heteroscopes, described a very interesting phenomenon (which is to be found on page 6). This observation, Mr. Dixey thought, showed clearly that the sense of fusion is not merely a matter of muscular adjustment of the eye, but that there is at the same time "a fusion elasticity of the higher cerebral centres." He did not know whether this matter had been further investigated, but he thought it very suggestive indeed, and that further investigation would be both useful and valuable. It was interesting in reference to the fusion of different sized retinal images, and afforded an explanation of a curious phenomenon which he (the speaker) had formerly explained as purely psychic. Dr. Ettles' physiological explanation

was much more acceptable, and the observation was interesting in its application to the treatment of anisometropia where lenses of different power created retinal images of different sizes. It would be interesting, also, to know whether the limit of fusion power had any pathological significance, and there was the question of estimating distance by stereoscopic means. The speaker was glad that Dr. Ettles had called attention to the question of atropine, to the possibility of error in estimating refraction when the pupil is widely distended. The basal zone of the cornea had a different refracting power to the central or useful part, and when the pupil was dilated by means of atropine, there was a certain amount of confusion in the retinal image. Mr. Dixey did not quite understand Dr. Ettles' advocacy of a 5 mm. aperture in front of the light in retinoscopy (p. 321 O.P.T.R.). The light emanating from an aperture of 5 mm. would not issue in parallel beams, but would be divergent, and when refracted from the plane mirror on to the pupil would cover a very much larger area than that of the pupil. What seemed to him to be Dr. Ettles' apparent object would only be reached by a diaphragm in front of the patient's eye. With regard to Dr. Ettles' suggestion of round eyes, he preferred the round oval form; and as for cylindrical lenses being cemented into the frame, that was all right so far as it went, but it did not altogether eliminate the danger of rotation. With regard to the expense of prisms, useful spherical lenses of prismatic effect could be supplied in most of Dr. Ettles' cases without any extra cost, the ordinary hospital price for a pair of spectacles, which is 2s. 6d., should be sufficient. The only thing to stand in the way being that if the Optician was not sufficiently skilled to set the lenses, he would have to get somebody more skilled to do it for him, but he thought that that would not apply to any member of the Optical Society. Referring to the latter portion of the lecture, Mr. Dixey said he was pleased to see that Dr. Ettles had no sympathy with the recent demand that school children should be sent to Opticians for treatment. He could not understand a man who could read Dr. Ettles' paper intelligently, at the same time advertising that he had a patent method of his own for curing squint by means of spectacles. Such cases, the speaker had no doubt, should be treated by men with medical

knowledge—not gained merely from text-books, but by medical training and experience. As regards the question of adopting a standard of visual acuity as a means of limiting the practice of the Optician, Mr. Dixey gave, as an illustration, a case of incipient glaucoma, which had recently been brought to his notice, where the visual acuity of each eye was 6/6.

Mr. Heywood asked whether Dr. Ettles would cover the sound eye of a squint for a prolonged period, citing a case which had come under his notice and been erroneously treated at a certain hospital. Mr. Clarke also mentioned a case where wrong diagnosis had been made at a hospital and Mr. Dadd asked up to what age did Dr. Ettles think operation to be of any use for squint.

Dr. Drysdale said, in connection with Dr. Ettles' lecture, that he was particularly interested in the mechanical models with which the subject was explained. With regard to the little point raised by Mr. Dixey in the question of printed matter, Dr. Drysdale mentioned the work of the eminent Dr. Chunder Bose, who has shown that all materials that we generally speak of as "dead," really behave after the fashion of living things, and quoted the example of a suspended wire with one end dipped in an acid solution, which set up a current somewhat similar to nerve movements. Another experiment, this one being optical, which Dr. Drysdale mentioned, was that if you put on a pair of spectacles, one eye of which was composed of a red glass and the other of a green glass, if the glasses are properly adjusted, one sees a white light. But this generally lasts only for a short period, and the usual thing is to see alternately a pale rose or light green. After a few more words as to the sight testing controversy, Dr. Drysdale asked Dr. Ettles to be good enough to reply to the questions put forward.

Dr. Ettles, at the outset, expressed his hearty thanks to the members for the kind reception they had given to his paper. His own impression was that he had put his foot into it, but in self-justification he would plead that it was his first appearance on the Optical Society's platform. In referring to Dr. Drysdale's remarks about Dr. Chunder Bose, the speaker stated that he had not studied that gentleman's works, but that what the President had said had interested him very much, and he

would not fail to read them in the near future. Referring to binocular vision he did not think that we really understood it yet, and quite a lot of work had to be done before they would be able to dogmatise on the conditions governing its manifestation. In microscopical work, Dr. Ettles mentioned that one gets into the way of working with both eyes open. Curiously enough, now and then the view changes and one sees all the brass work and screws instead of the object under examination. He supposed that one of the questions which they would like to solve was whether alternating vision, which we do know goes on, would account for the occasional manifestation of a squint. Personally, he did not think so, because this vision, so far as he had observed, takes place with great rapidity. Even if monocular vision should persist for two, five, or twenty minutes, it would scarcely account for the long periods of the alternation of squint. The speaker mentioned a case of a child who only squinted after dinner, which he explained as being caused by the power of fusion refraction being diminished.

The speaker went on to consider the question of the status of the Optician, pointing out that there were very few other professions that had such a difficult task before them as Opticians had in weeding out the undesirables, and that at the present moment the good must suffer for the bad. He quoted the case of the person who comes up to a man at a country fair with an optometer in one hand and grabs him by the arm, as an example of the latter class. With regard to fusion, he had been experimenting as to why a single line is left, and had been using extremely fine lines, a hair line, in fact. He thought he could say without hesitation that the line remained single because there is overlapping of the edges. In regard to blurred images, Dr. Ettles stated that if they observed two persons both of 3 D., one having worn glasses and the other not having done so, the latter would have a much greater degree of perception than the former, from which one might conclude that there must be a higher education of the fusion. In reply to Mr. Dixey's request for an explanation as to the use of a Thorington's chimney, Dr. Ettles said that quite a number of workers had found that the basilar portion of the cornea was flattened and that there was a spherical cap

which was the seat of accurate refraction. In using a small point of light, a small retinal picture is obtained which is observed in skiascopy, and by observing its motion one can study the spherical cap of the cornea. As regards the cheapness of prism spheres, that was a point on which he (Dr. Ettles) was perfectly ready to bow to the superior knowledge of Opticians, and he was grateful for the little look behind the scenes of the optical profession which had been accorded him. The speaker regarded the correction of a normal presbyope to be as safe in the hands of an Optician as in the care of a medical man, and spoke at some length on the relationship of the Optician to the medical man. In replying to the questions put forward by Mr. Clarke and Mr. Heywood, Dr. Ettles thought that the case of the treatment of the squinting boy of five until he was ten by atropine alone, indicated an absolute ignorance of the pathological issues. He could not understand how the case had been permitted to go on for five years. In regard to the relative value of operation for squint, he thought it never produced a perfect result. As to when to recommend operation: if one had tried all the optical means at one's command, and the squint had not disappeared after a year or two of treatment, and if the patient was impatient and wanted the deformity removed, then one could safely suggest operation. But they should not look for a cure, because he did not think that operation could in any case effect a return to binocular vision.

THE FUNCTIONS OF TRICOLOUR FILTERS.

PAPER BY A. J. BULL AND A. C. JOLLEY.

Read before the Optical Society on December 17th, 1903.

PART I.

ALTHOUGH a great deal of theoretical work has been advanced upon the subject of the filter curves of tricolour filters employed for taking the negatives of tricolour photographs, there seems to be still a considerable difference of opinion on the subject. Examination of the filters at present on the market seems to point to the fact that too little attention is given to the point of the overlapping in the construction of such filters. The object of this paper is, therefore, to consider the effects of the overlapping and to show how this will affect the final colour reproduction.

We are not here dealing with the limits of photographic action, either at the red end of the spectrum (which is practically always limited by the sensitiveness of the plate), or at the violet end; neither are we dealing with the shape of the curves of photographic action, or the luminosity of the various spectrum colours. In passing, we should note that the curve of photographic action never corresponds with the visual effect, and the range of gradation which even the best dry plate will record is less than the eye can perceive.

One effect of this compression of the scale of gradation is to make the curve of photographic action steeper than the visual at the places where the filters absorb, and this, combined with the fact that the dyes generally used in making the filters have themselves abrupt absorptions, tends to prevent any gradual shading off at the limits of the filter records. This steepness is, moreover, enhanced in making the necessary chromated gelatine reliefs for transparency and paper work. We are not now attempting to put forward any final definition as to what tricolour filters should do, but merely suggesting a first step in their adjustment.

We consider that there is ample justification for our doing this, if only from the performance of filters now

on the market. Some workers have thought that the curves of photographic action given by the tricolour filters should bear some direct relation to the curves of the colour sensations. They would, therefore, have excessive overlap, and we think we shall be able to show that any such filters will give too great a degradation of the reproduction colours. The only writer, as far as we know, who lays any stress on the amount of overlap is Professor Miethe, who regulates its amount by a somewhat rule of thumb method.

In our tests upon the various filters we have examined we have preferred to use a fairly pure spectrum rather than trust to the, perhaps, doubtful effects of the impure colours of any colour sensitometer. The use of the spectrum was further necessitated by our wishing to

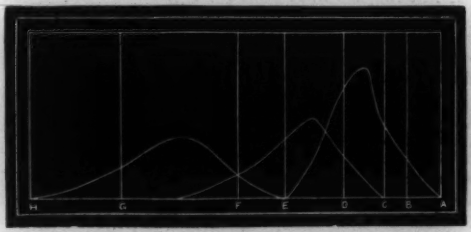


FIG. 1.

measure the region of the overlapping. We consider that if any process of colour photography will give a fair reproduction of a spectrum, then it will reproduce any more complex colours well, since it reproduces their components, and we have taken this as the severest test we can apply. We believe this view is also held by Mr. F. E. Ives. Let us, here, carefully distinguish between the colour *sensation* curves, generally called "Maxwell's curves," and colour *mixture* curves. Maxwell's curves are not, as has sometimes been stated, curves obtained by matching each part of the spectrum by three pure monochromatic lights—these may be more properly called colour mixture curves (and were drawn by Maxwell)—but those usually known as "Maxwell's curves" give for every point of the spectrum the relative amounts of the

three sensations which are stimulated in the eye, and which together make up the visual effect produced.

Some experimenters have taken the colour mixture curves, and others the colour sensation curves, as the basis of their theoretical filters and inks. Whatever may eventually be found to be the best practical curves for the filters and their printing colours, we think we may state the two following fundamental points :—

First.—That since in any photographic process one prints from the parts of the negative where the light has not acted, or in proportion as the light has not acted, each printing colour should consist of white light, less the colours recorded through the filter.

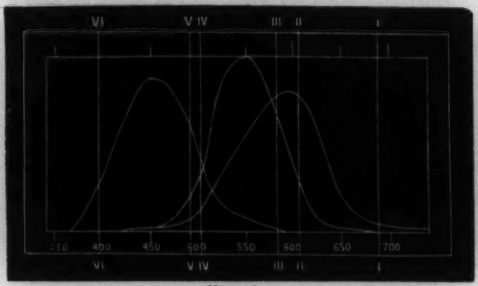


FIG. 2.

Second.—That the regions where the photographic records overlap should accord in hue with the printing colours of the red and blue negatives. It is with this latter point we chiefly wish to deal in this paper.

It would be well, in passing, to examine a few of the definitions and tests that have been proposed by the various experimentalists in this subject. Sir W. Abney* states that the filters should follow the sensation curves and that the printing colours should be the exact complementaries of these, although in a paper before the Royal Photographic Society† he discusses the use of inks, and, hence, filters that have abrupt absorptions.

* "Instruction in Photography."
† 'Journal of Photography.'

He proposes as a test for the filters, a colour sensitometer, in which the colours have been measured in terms of three monochromatic primaries; these are graded in sets so that each set contains a relatively equal amount of its own primary. A screen and plate are said to be adjusted when a photograph of its graded sensitometer gives uniform densities.

Now, we consider any colour sensitometer method such as this likely to give misleading results, because it would answer with filters passing only narrow bands, whereas, as Mr. Ives has pointed out, such filters would not render the colours whose regions of maximum luminosity happen to coincide with the colours not recorded. Moreover, the adjustment of filters by such a method would give no indication as to the form of their curves of action, and it would appear that the intention is for them to be adjusted to the sensation curves.

Mr. F. E. Ives, in the third Trail Taylor Memorial lecture, states:—"That the distribution of photographic action should correspond to Maxwell's *colour mixture* curves," and that the printing colours should be antichromatic to these.

Mr. E. Sanger Shepherd, in the Cantor lectures of 1900, after discussing the use of Maxwell's colour mixture curves, states that it is better for the filter records to follow Sir W. Abney's sensation curves, and proposes a sensitometer similar to that previously described as a test.

Mr. Howard Farmer states that it is best in practice to simply divide the spectrum into three bands, having no overlap, the junctions being at about the D line and a little beyond the F line in the blue.

Dr. Miethe, of Charlottenburg, has proposed the use of filters similar to those of Mr. Farmer, having no overlap, and dividing the spectrum at about 6,000 and 5,000, but states that these would give worthless results unless used in such a manner that an artificial overlap is produced by giving a second and shorter exposure with the next filter on the same plate. For instance, for the red record, he gives exposures through both red and green filters in the ratio of perhaps 15 to 1.

It will be seen from the above authorities that there is a great discordance of opinion as to what three-colour filters should do, and it will, therefore, be interesting

to consider in detail how each spectrum colour may be reproduced by the tricolour process.

We may divide the spectrum as far as its photographic reproduction is concerned, into five regions, that is without taking into account any gradual shading off of the filter records at their limits. These regions are, firstly, those where only red, green, or violet, are recorded through their respective filters, and which are reproduced by the superposition of the printing colours in pairs. And, secondly, those which are recorded through the red and green, and through the green and violet filters, and which are reproduced by the printing colours of the violet and red records alone.

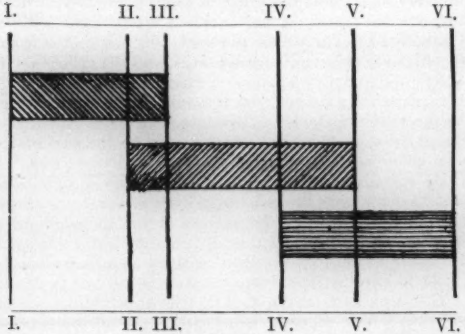


FIG. 3.

Let us suppose in fig. 3 that our red filter and plate records from the beginning of the visible red I. (λ 6900) up to III. (λ 5820) at the end of the yellow, and, as from this negative we shall print upon the regions not recorded, let us print with a colour which consists of the summation of all the colours from III. (λ 5820) to the violet end of the spectrum (λ 3970). The hue of the colour obtained by combining all these wave lengths will be a greenish-blue.

Suppose, further, that our green record is from the beginning of the yellow II. (λ 6070) to the end of the

blue-green IV. ($\lambda = 4920$), and that from this is printed on the parts not recorded, *i.e.*, on the red region I. to II. and the violet region IV. to V., a colour consisting of the sum of the colours from I. to II. and from V. to VI., this will be a crimson.

Again, we will suppose the violet record extends from IV. (λ 5020), the beginning of the blue-green, to the end of the violet VI. λ (3970); the printing colour for this will consist of the remainder of the spectrum colours, viz., from the beginning of the red I. to IV., this is a yellow.

Let us now superpose the three prints and consider the nature of the reproduction colours. From I. to II. we have superposed the crimson and yellow, and we have to note what part of the incident white light will be transmitted. The yellow pigment will absorb the violet to which the crimson is transparent, and the crimson will absorb the green and yellow to which the yellow pigment is transparent; the common transparency will, therefore, be the region I. to II. Therefore, this region is reproduced by a uniform hue which is due to the sum of its own colours. This colour is a slightly impure red.

By reference to the sensation curves, fig. 2, it will be seen that not only the red, but a little of the green sensation is stimulated by the shorter of the included wave lengths, but, in spite of this, it is a sufficiently good red for the purpose, because it is as pure as any red one is likely to have to reproduce.

The second region II. to III. will have printed on it only the yellow pigment which transmits all the colours up to IV. ($\lambda = 5020$), and, on referring to the sensation curves, it will be observed that the spectrum yellow which stimulates only two sensations, the red and the green, is being reproduced by a colour, the components of which stimulate not merely the red and green, but also a little of the blue sensation, so that the spectrum yellow is reproduced with the addition, not of real white, but the sensation of white.

The yellow and greenish-blue pigments are superposed from III. to IV., and their common transparency corresponds to the same region, which is a green of slightly less purity than its central part. The greenish-blue, which reproduces the overlapping part IV. to V., stimulates the red sensation rather more than do the colours

of the corresponding part of the pure spectrum. Finally, the region V. to VI., on which is superposed the crimson and greenish-blue pigments with a common transparency to the colours of the same region.

We might have, by integrating the areas of the curves cut off by the dividing lines in fig. 2, found the percentage composition of the reproduction colours in terms of the sensations, had we thought it worth the while. It will be noticed that we have drawn no distinction between the blue and violet of the spectrum, pure violet is a colour which one has practically never to copy. It is thus seen that in reproducing a spectrum we do so with slightly degraded colours, and as Dr. Clay has pointed out, this effect is least noticeable when the degradation is with white.

But we now wish to point out that this is immaterial, for when we come to consider the reproduction of the colours one meets with in nature and in pigments, this degradation with white in nearly all cases ceases to exist. Take for instance the reproduction of a yellow pigment. All yellow pigments transmit or reflect the red, yellow, and green of the spectrum, and some even a little violet, that is to say, they are already equivalent to a spectrum yellow diluted with white, and are, therefore, as impure as the colour with which they are imitated.

There are few greens to be met with whose spectra do not extend over a greater range than from λ 5820 to λ 5020, and so in their reproduction by the above system there is no addition to their impurity. One might cite many cases, but the whole point turns on the fact that although one may turn to the spectrum as a test, one does not in practice have to reproduce pure spectrum colours.

Considered from the point of view of the aforementioned treatment, the tricolour process by negative synthesis would appear to depend only indirectly upon the tricolour theory of vision, for the colours are approximately at least imitated independently of the mechanism of vision. It appears from the foregoing that the region which is recorded through both the green and the red filters is reproduced in the printing colour of the violet record, viz., a yellow; and that the region recorded through both the green and violet filters is

reproduced by the printing colour of the red record, generally a blue.

Now, as one has, at present, to use reproduction colours which seldom give us what we require, we suggest that the amount which the red and green records fully overlap should be restricted to the region that corresponds most nearly in hue with the printing yellow to be used. Similarly, the completely overlapping region of the green and violet records should be confined to the part most nearly matching the blue or greenish-blue printing colour. We consider the overlapping should never exceed this, and on the other hand should never fall far short of it.

The gradual overlaps required by filters, based upon colour sensation curves, or intended to follow colour mixture curves, are seldom or never realised in practice. They are things which are talked about but seldom produced. Mr. Ives is the only person, as far as we know, who claims to have successfully used filters whose records correspond to colour mixture curves. We must give deference to Mr. Ives' statements, as he is a practical as well as theoretical worker. But some photochromscope filters of his which we have examined only give a very crude approximation to colour mixture curves, and their excessive overlap is, we have found, fatal to their use for three-colour printing.

There is also another point which seems to have been lost sight of altogether, and is, moreover, an objection which has not as much significance if the filters have fairly abrupt absorptions. In all molecular phenomena, *i.e.*, in magnetism and electric response to stimulus of living or non-living bodies, in photographic action, etc., there is a period of ineffective stimulus, *e.g.*, in the case of a photographic plate there is a certain minimum amount of light action below which no deposit of silver occurs.

Suppose now that by a certain exposure to a spectrum with suitable filter and plate, one obtained a silver deposit whose density corresponded to, say, the red colour mixture curve; if we gave any other exposure, then the shape of the curve ought to remain the same (except, perhaps, with great over exposure, when in any case all parts would tend to reach their maximum limit of density). But this would not be so, on reducing the

exposure, the less dense parts would lose their density too rapidly and soon become clear glass while there was

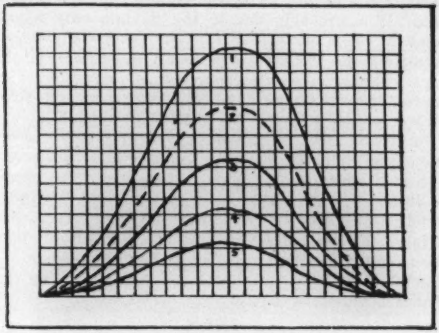


FIG. 4.

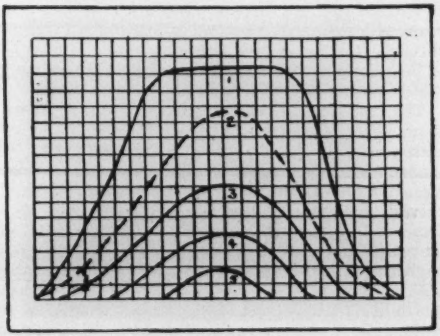


FIG. 5.

still a good deposit in the densest part. Fig. 4 shows what ought to occur on varying the exposure from the original, and, fig. 5, what does actually occur.

This shows that colour mixture and sensation curve filters will only work as they are intended to do when all parts of the subject to be copied have the same brightness. It is probably due to the maxima only having acted in some parts of the picture that the previous example shown was not worse than it is. Of course, this argument applies to all filters only in greatly varying degree, but filters having marked maxima will act differently in copying light or dark colours.

In view of the rapid growth of the application of tricolour, especially to photomechanical processes, it is a matter of commercial importance, as well as of theoretical interest, to know what one wants, even very approximately, in the construction of the filters and the selection of inks. So confusing is the present state of affairs that few tricolour operators in the process trade have any idea of what they require, and they are guided by rule of thumb considerations only.

Part II.

We have examined several sets of commercial and recommended filters, by reproducing a fairly pure spectrum with them. The regions of the spectrum recorded through them, and also the approximate distribution of the colours in the reproductions, are given in the two tables accompanying this paper.

The reproductions of the spectrum, we are about to show, were made with superposed gelatine reliefs, dyed with erythrosine, brilliant yellow, and fast green (blue shade). The peculiarities of the reproductions made by means of the various filters are as follows:—

Ives' Kromskop Filters.—These are used with the Lumière C. panchromatic plate. It must be noted that no red filter records the first half of the red between the beginning of the visible and about λ 6500. This is, therefore, always reproduced as black. The very moderate sensibility of all photographic plates to light of this region, allows of its use as a safe light for their development in the dark room.

The gradual shading off of these filter records into each other, produces a more pleasing effect in the reproduction than the abrupter absorptions of the other filters. It may be assumed from the writings of Mr. Ives that

these filters are intended to give in the negative photographic records such that the variations in the density of the silver deposit approximate to the colour mixture curves.

The result of this excessive overlap is that the part which is reproduced as yellow, instead of being confined

FIGS. 6 AND 7.

to the yellow itself, spreads as far as λ 5300 in the green, so that the greater part of the green is reproduced as yellow. The overlap in the blue is not excessive unless a greenish-blue pigment is used for the printing, when the deep blue of λ 5000 to λ 4750 is reproduced with a greener hue than the original

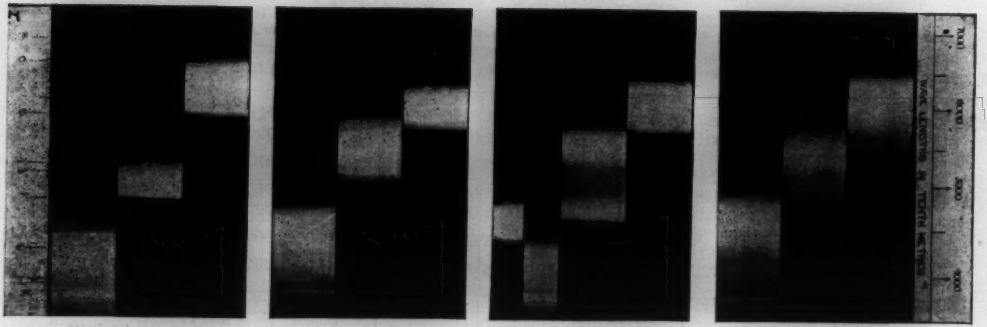
TABLE I.—Showing the Regions of the Spectrum Recorded Through Certain Tri-colour Filters.

FILTERS.	RED RECORD.	GREEN RECORD.	VIOLET RECORD.
Ives (Kromskop)	6520 to 5250 (maximum effect 6480 to 5800).	6300 to 4700 (maximum effect 5700 to 5250).	5050 to 3800 (maximum effect 4950 to 4500).
Farmer and Simmons	6500 to 5700.	5850 to 4550 (with a minimum 5300 to 4900).	No, 1. 4500 into ultra-violet No. 2. 4850 to 4350.
Sanger Shepherd	6340 to 5710.	5900 to 4950	4820 to 3800.
Lumière	6640 to 5860.	5420 to 4800	4730 into ultra-violet.
Lumière N.A. Co.	6580 to 5760.	5800 to 4660 (maximum effect 5800 to 4900), also slight effect in ultra-violet 3890 and beyond.	4700 into ultra-violet.
Klein (for collodion emulsion)	6470 to 5470.	5900 to 4950	Chief effect—5100 to 4500, tailing off to 3800. Slight effect—6000 to 5830.

TABLE II.—Showing Approximately the Regions of the Spectrum which are Reproduced in the Colours Indicated.

FILTERS.	UNRECORDED RED (REPRODUCED AS BLACK).	RED.	YELLOW.	GREEN.	BLUE OR GREENISH-BLUE.	BLUE-VIOLET.	BLACK.	WHITE.
Ives.	6900 to 6520	6520 to 5800	5800 to 5300	5300 to 5000	5000 to 4750	4750 to 3800		
Farmer & Simmons No. 1.	6900 to 6500	6500 to 5850	5850 to 5700	5700 to 4500	4500 to 4550	4550 and beyond.		
Farmer & Simmons No. 2.	6900 to 6500	6500 to 5850	5850 to 5700	5700 to 4850	4850 to 4550	4550 to 4350	4350 to 4000	
Sanger Shepherd.	6900 to 6340	6340 to 5900	5900 to 5710	5710 to 4950		4820 to 3800	4950 to 4820	
Lumière.	6900 to 6640	6640 to 5860		5420 to 4800		4730 and beyond.	5860 to 5420 4800 to 4730	
Lumière N.A. Co.	6900 to 6580	6580 to 5800	5800 to 5760	5760 to 4700	3890 to 3550	4700 to 3890	4900 to 4730	
Klein.	6900 to 6470	6470 to 6000	5830 to 5470	5470 to 5100	5100 to 4950	4950 to 3800		6000 to 5830

Note.—For comparison with the figures in this table it may be interesting to note the positions of the various colours in the spectrum. From the beginning of the visible, about λ 6900, to λ 6200, is a pure red. From this point an orange tint is gradually assumed until it becomes a distinct yellow at λ 5900 to λ 5780. The green region lies between this and λ 4900, there being a gradual change from a yellow-green to a blue-green. From λ 4900 to λ 4600 is a deep-blue, and from λ 4600 to the limit of visibility, about λ 4000, a violet.



LUMIÈRE OF LYONS.

SANGER-SHEPHERD.

HOWARD FARMER AND GUY SIMMONS.

F. E. IVES. (KROMSKOP).

Lumière N.A. Co.

H. O. Klein.
(Collodion Emulsion).

Bierbrich Scarlet.
Napthol Green
Methylene Blue.
} Ethyl-red Plate.

Scarlet R.R.
Napthol Green.
Methylene Blue.
} Lumière "C" Plate.

Messrs. Farmer and Simmons' Filters for Process Work (see 'Photographic Journal,' June, 1901).—These are given in solution for use in ½-in. cells. Two blue filters are given. BLUE FILTERS, No. 1.—Eosin (blue shade) solution used with a Mawson lantern plate. This records the violet and ultra-violet only. BLUE FILTER, No. 2.—Eosin and potassium chromate solution. This cuts out the ultra-violet and part of the violet.

GREEN FILTER.—Potassium chromate and methylene blue. This is used with a Lumière A plate. The broad even band of colour, transmitted by this filter, allows the band of comparative insensibility in the blue-green, possessed by this plate, to become rather marked. The result is that a little red prints on this part and causes a dulling of the colour.

RED FILTER contains potassium bichromate, eosin and ammonia, and it is used with a Lumière B plate.

With these filters the part that is reproduced yellow is in the yellow-green, there is the degradation of the blue-green noted above, and with the second blue filter the extreme end of the violet is reproduced as black, but as its luminosity is low it probably is not of much consequence.

Sanger Shepherd's Filters for the Spectrum Plate.—The photographic records with these filters end somewhat abruptly. The red is not recorded quite as far as with the other filters; this is due to the peculiarities of the plate used. The band reproduced as yellow is too broad, extending from the truer yellow into the green at λ 5710, while in the blue there is a narrow band of black.

Filters from Messrs. Lumière, of Lyons.—The red, green, and blue filters are used with the Lumière B, A, and extra rapid plates respectively. They give three isolated bands of red, green, and blue-violet only, of fairly correct hue for the positions they occupy, but the intermediate parts are reproduced as black, as they are not recorded at all. The ultra-violet is reproduced as blue-violet.

Filters from the Lumière N.A. Co.—Used with the same plates as the last. The reproduction with these is fairly good, except that all the colours are shifted a little towards blue end. The narrow band of yellow is in the

yellow-green, and the green extends into the blue. The slight transparency of the green filter to ultra-violet causes a region there to be reproduced blue.

Klein's Filters for Collodion Emulsion.—These are used with Albert's collodion emulsion. For the red filter, the emulsion is sensitised with Penrose's "C" sensitiser, and for the green and blue filters with Albert's "A" sensitiser. The most peculiar point about these filters is that the blue one consists of a dye that absorbs green only, and as the sensibility of the "A" sensitised emulsion extends into the yellow, this colour is slightly recorded through the blue filter, and being also recorded through the other two is reproduced as white. The part that is reproduced as yellow is the less refrangible half of the green. The remainder of the spectrum is more correct.

It occurred to the authors that it ought not to be difficult to make a set of filters that on a good panchromatic plate would give a reproduction as good or better than any of these. We, therefore, dyed up a set of dry filters with methylene blue, napthol green and scarlet, and these, when roughly adjusted, gave on a plate dyed with Miethe's ethyl red, the following spectrum records:—

Red filter	... ...	6500 to 5850
Green filter	... ...	5900 to 4650
Blue filter	... ...	5000 to 3600

We found that these gave a fairly good, but not by any means perfect reproduction of the spectrum; the overlapping portions being here restricted approximately to the hues of the yellow and blue printing colours, and there being no stray portions of light coming in where they are not wanted.

The last set of records that we have to show emphasise two points already mentioned. The filters are methylene blue, napthol green, and scarlet R.R., used on a Lumière C plate. The red filter was adjusted visually and well illustrates the fallacy of such a method, for, as will be seen, its photographic record shows that it is transparent to light beyond the visible violet. The green filter has been underexposed, and shows as a consequence the degradation of the green with red, due to a band of insensibility in the plate and a black band in the yellow.

The degradation of greens is a matter of great importance in tricolour work. It is of constant occurrence and there are several causes tending in the same direction.

(1) Excessive overlap between the red and green filters, whereby an insufficient amount of blue occurs in the print.

(2) The comparative insensibility of most orthochromatic plates to blue-green, which results in an excess of red being printed on them. This is really a case of the filter record having maxima; a point we have discussed.

(3) The want of transparency to green of most of the blue printing colours, and their slight transparency to red.

To summarise our suggestions:—

(1) Printing colours should be transparent to the colours not recorded by their respective filters, and are not to be merely complementaries (except that the blue printing colour must not transmit the extreme red which is never recorded).

(2) Complete overlapping regions to accord in hue with yellow and blue printing colours.

(3) Records to be without maxima (or minima) over the regions where complete record is required.

Discussion.

THE PRESIDENT thanked the authors for the extremely interesting paper they had contributed. He was afraid it was a subject somewhat outside the range and experience of many present that night, but, nevertheless, it was one of great importance. As they had seen for themselves, there was an enormous field open for research in three-colour work, and he especially commented on the fact that the authors had taken the most severe test of all, namely, the reproducing of the spectrum by means of the various filters.

In spite of the stringency of the test, it was by such methods that progress was made. He congratulated the authors on their success in obtaining such purity in the green, and remarked on the enormous amount of work entailed in the preparation of the experiments shown, for he did not think there was any subject which entailed so much work as colour reproduction process. There were some visitors present who had taken a special

interest in the matter, and he had great pleasure in calling upon Dr. Clay.

Dr. Clay expressed his great pleasure at being present, and stated that when he saw the printed copy of the lecture, which had been forwarded beforehand, he could not resist the temptation to leave his work and be present that evening. He fully and heartily endorsed the President's words in regard to the enormous amount of difficulty experienced, and time which the authors of the paper must have expended. He was especially pleased and astonished with the admirable way in which both the actual spectrum and the reproduced spectrum were thrown on the screen side by side, exactly in the same size and the same position, so that the audience might be able to judge by comparing the two.

There was one point in the paper that he did not quite agree with, and that was where the authors stated that the degradation of the white was immaterial. He agreed with their meaning, but he did not think that they had expressed themselves quite correctly. He himself thought that it was because the degradation of the white did occur that the filters they had shown reproduced, in some cases, the spectrum so badly. So badly, in fact, did some of them reproduce the spectrum, that one would imagine they could not reproduce natural objects anything like correctly. And yet they did this, as many of those present had perhaps seen.

The Sanger-Shepherd filters, for instance, gave remarkably good results, in spite of the fact that Messrs. Bull and Jolley had thrown on the screen a spectrum taken through the Sanger-Shepherd filter, which certainly did not look like the original. And he thought that the reason of this was that the colours in the spectrum itself were sharply set out, which, in the natural objects, were spread over a greater area. By widening the slit in the spectrum, Dr. Clay thought it was very probable that one could produce artificial colours of which the position was known, on which one could, therefore, work scientifically, and which wore some resemblance to the natural colours in that they were impure colours.

So that, he thought, exception should be taken to the authors' statement that the degradation of white was immaterial. He thought, therefore, that to judge the

results of the various filters by a pure spectrum was not only a very severe test but, perhaps, not the best way of judging of their effect when they were used for reproducing objects composed of impure colours. With regard to overlapping, that was a very difficult question. At the same time, it seemed to him that, although in all probability filters which, in copying the spectrum, produced overlapping, as the filters shown in fig. 8 (see pp. 44-45) would, in copying the actual object, which consisted of very impure colours, probably give a rather high percentage of white.

He should like to test this for himself, but imagined there would occur a larger proportion of white than was really necessary. However, practice was better than theory, and, as he was not a practical man in these things, he was willing to abide by the experiments of Messrs. Bull and Jolley.

Mr. Gamble said that he had learnt a great deal from being present that night, for the authors of the paper had presented the subject to him in a totally new light, although he himself had been studying it for many years. He, too, eulogised the way in which they had shown the results in the actual colours, especially the spectrum records. Some year or two ago there was a very heated discussion in the photographic press as to whether the spectrum should be reproduced by overlapping or by "chunks"—as somebody had termed it—and he thought that if they had had the opportunity of seeing the results presented to them that night, the settlement of that discussion would have been much hastened.

One photographic authority had once said that it was no good copying the spectrum as a guide to practical three-colour work, because, in practice, if you went out with your camera to photograph nature, you would not find the spectrum hanging on the trees. However, he thought that it was, at all events, necessary to work on the spectrum, even if you had to leave it at home. One thing which was especially valuable in the paper was that the various records were given in wave lengths. He did not think that this had been done, to any extent, before in three-colour literature, and it now brought it down so that anybody could follow it.

The three-colour process, as it was known to-day,

bristled with contradictions, every authority seeming to contradict either himself or some other authority, and causing great bewilderment. He should very much like to see the three-colour process brought down to a scientific basis, such as had been outlined that night. Among the things remaining to be done was the making of a plate sensitive to green. There remained also much to be done respecting the lack of the correct inks and the influence of exposure on the relative colours. It was well known that, in photographing a colour subject, totally different results could be obtained according to the exposure given. Another point was, that the violet light was not sufficiently emphasised, and, in some cases, in most, he might say, entirely neglected by those making up colour screens.

He quite agreed with Dr. Clay with regard to the point that the spectrum was, perhaps, not the best test for filters, and this fact was brought home to him in regard to the results which were shown from the Klein filter. Mr. Klein, he said, had found himself unable to be present, but on talking over the paper with him (Mr. Gamble) he had pointed out that the blue sensation filter, to which the authors referred, was a filter which had been discarded for nearly 18 months. It was originally prepared to suit a collodion emulsion plate, the maximum sensitiveness of which lay between E and G, having no sensitiveness in the yellow at all.

Since the introduction of the new sensitiser, which possesses a maximum in the yellow-green, a new filter had to be prepared to avoid any action taking place in the yellow. This filter was not blue, as the authors had described it, but of a bluish-red, best specified by the shade of an aniline dye of the Rhodamine group, and its absorption band affords full protection against the slightest action of the yellow, yellow-green, and blue-green. In concluding, Mr. Gamble complimented Messrs. Bull and Jolley on the pains they had taken to bring their subject before the Society in such a practical, as well as a scientific manner, and stated that should the paper be published it would be very valuable to the advanced workers in the trade.

Dr. Walmsley did not agree with Mr. Gamble as to the photographic authority, who said that the spectrum was not to be seen hanging on trees. He had himself

seen it, but he did not like to do so for it was always in rainy weather.

Mr. Ryland thought that the optical work necessary must be perfect in order to give the colours in their due proportions, and that, therefore, if the work went forward at all, it meant more work for those who were investigating lenses.

Dr Ettles spoke warmly of the lecture and the demonstrations which had accompanied it. He yielded to none in his admiration of the work of Messrs. Bull and Jolley. He had regarded the subject more or less from a physiological point of view, and it seemed to him that tri-colour photography bore out the views of the great worker, Young, who first suggested that we had three primary colour perceptions, red, green, and violet. Since then a furious war had been waged round Young's views, but it seemed to him that the work of Messrs. Bull and Jolley went largely to substantiate Young's views—that we perceive colours by having three primary colour sensations of red, green, and violet.

It was interesting to remember that the periphery of the retina was only conscious of two colours—blue and yellow. Dr. Ettles here mentioned the fact that if a red disc is taken and looked steadily at, it would soon begin to darken, and after a few minutes it was entirely impossible to tell what colour it was. The explanation was that the retina became exhausted of that colour. This fact might be made use of, if it were desired, to test the value of a colour, say, a red. Its complementary colour ought to be regarded, a blue-green for example, and the retina would then be exhausted of that colour when the correct chemical value of the red would be seen.

He had himself tried that experiment and succeeded. It had been suggested that the rods and cones of our retinas had different values. Dr. Ettles, in concluding, said he felt that most of the theories which had been put forward were simple statements of facts under the cover of theories, while others had studied phenomena rather than explained them, a difficulty which all theorists were liable to fall into.

Dr. Drysdale said in respect to the testing of the spectrum, his point was that if one could reproduce the spectrum satisfactorily it would be possible to reproduce

anything. On the other hand, he did not deny that the spectrum was a very stringent test, and it might be possible to have filters and screens able to reproduce other objects fairly well, and yet not able to reproduce the spectrum perfectly. Another point was the question of the colour sensation of curves. If filters were going to do anything it was upon the *colour mixture* curves that one must work. There were three-colour sensations in the eye. How this came about it was not necessary to say then, but if they were going to reproduce colours by the three-colour process the method must be that three photographs of the object must be taken, and each of these photographs in turn must be made to take one of the colour sensations of the eye.

How far this was done by orthochromatising the plate, or by means of coloured screens, was only a matter of practice. The question of the reproduction of the colours afterwards was a more complex one, and it was there that the great divergence existed. It was perfectly true that, in many cases, they got degradation by having filters too overlapping, but that was, he understood, because proper reproduction colours had not been employed. And under these circumstances the remarks made by Dr. Clay did hold to a certain extent. Another very important question was that of the length of exposure. It had been shown that even if perfect colour reproductions were successfully obtained, and the plates and screens and everything else were correct for a subject with certain luminosities of the various colours, another subject with different luminosities would not be correctly reproduced.

This fact, Dr. Drysdale thought, had not been realised to anything like the extent to which it should have been. It probably marked the limitation which could not be passed with plates in their present state. The question of wave lengths was a point which ought to have been known years and years ago, and yet it was undoubtedly a fact that very few workers had expressed their results in wave lengths. He was looking forward to the scientific working out of the three-colour process. Another and simpler aim, which was even possibly of greater commercial value, was that the actual expression of colours should be in terms of the colour sensations. He looked forward to the time when all the drapers' shops,

instead of using such terms as "crushed strawberry," etc., would have a simple apparatus, which he believed could be made, in which a piece of dress fabric would be put in front of a slit, and the knowledge ascertained that it was, say, 30 per cent. red, or 40 per cent. green, etc.

A member of the audience here explained that the apparatus referred to was already a *fait accompli*.

Dr. Drysdale was very pleased to hear it, and concluded by calling Mr. Bull to reply to the discussion.

Mr. Bull, in the first place, expressed his thanks for the way in which the paper and the experiments had been received. The point touched upon by Dr. Clay, with regard to the spectrum as a test, was an interesting one. He thought it might be assumed that if one could reproduce each colour upon its own merits, if one could reproduce the spectrum perfectly, then the more complex colours could also be reproduced.

He preferred to reproduce the spectrum, as Mr. Gamble put it, in "chunks." It had been shown in the paper that if filters giving gradual overlapping were used, such as the Kromscop filter of Mr. Ives, then it would be possible, by testing the exposures, to reproduce the spectrum fairly well, but exactly how such filters worked depended entirely on the exposure given. Mr. Bull quoted the instance of using filters on an oil-painting, a portrait, for example. The face would be light and the background dark. For the photographic plate to record the detail of the dark background, the face would have to be somewhat over-exposed; that meant that both limits of exposure were being employed on that picture.

This being the case, the filters would be acting with tremendous overlapping, and that was the reason of the varieties obtained by different exposures, and why they did not think that much could be done with filters following sensation curves, or, rather, colour mixture curves. As Dr. Drysdale had observed, colour photography had no relations to colour sensation curves, the colour mixture curves being the only curve that could be used. In bright sunshine greater ranges of exposure would be obtained than in the oil painting.

He considered that it was necessary that filters should act in the same manner in copying the dark portions

of the picture as in copying the light. As all colours, which have to be reproduced both in nature and from pictures painted in pigments, gave broad bands in the spectrum, he thought that filters could be obtained which would correctly record the dark colours as well as the light, and that they would be better than the colour mixtures, even though they reproduced the spectrum in "chunks." He thought also that the mode of recording colours in wave lengths was the only rational one.

Referring to what Mr. Gamble had said, as to the ultra-violet light, if an ordinary plate were used without a filter, as was sometimes done, then a great part of the light recorded was not seen by the naked eye.

Mr. Jolley, and he, systematically cut out the ultra-violet in all their work. It had not a bad effect, and certainly could not produce a good effect. The point mentioned by Mr. Ryland, in regard to lenses, was one of considerable importance in tri-colour work, but he might mention that in process work one rarely worked at a greater aperture than 16/22, and with that aperture and a good rapid rectilinear lens, it is possible to get three images of the same size. Comparatively simple lenses were not barred from three-colour work, and Mr. Bull mentioned the case of a gentleman who used a Lancaster guinea camera, during a trip in Switzerland, and obtained a very fine set of views that registered perfectly, although the lens was only a single view landscape lens.

Dr. Ettles had raised an interesting point with regard to three-colour vision, but he would point out that where one reproduced a picture by what is known as positive synthesis, that the positives made from the negatives are seen by coloured lights and the three pictures are, by some optical contrivance, superimposed. Undoubtedly one could work out a theory from that based on the three-colour curves. Mr. Bull then showed a few slides on the screen, including one illustrating patches taken from a Chapman-Jones plate tester.

THE OPTICAL SOCIETY AND THE NATIONAL PHYSICAL LABORATORY.

PAPER BY PROFESSOR R. T. GLAZEBROOK, F.R.S., *Hon. Member.*

Read before the Optical Society, January 21st, 1904.

I FEAR that up to the present connection between the Optical Society and the National Physical Laboratory has not been as close as it might have been. I have to admit with regret that in our latest test pamphlet the following clause occurs: "The arrangements for optical tests are as yet incomplete, though some can be undertaken."

For this condition of things, I take some of the blame to myself, not all; when the Laboratory was first started an Optical Committee was appointed to advise the Executive as to the optical tests which should be commenced. The Committee consisted of Lord Rayleigh (Chairman), Sir Wm. Abney, the Astronomer Royal, Professor Clifton, Sir Howard Grubb, Mr. Dallmeyer, Professor Fitzgerald, Professor Joly, Mr. T. Matthews, the Earl of Rosse, and Professor S. P. Thompson, and their report was as follows:—

"The Sub-Committee on Optics note that navy telescopes, binoculars, photographic lenses, and sextants are already tested at Kew. They are of opinion that the work thus begun might be usefully extended. Thus, astronomical and terrestrial telescopes, other than those intended for the navy, and microscopes should be tested. The more remunerative work would be that of testing numerous instruments of moderate excellence. The question as to whether arrangements should also be made for testing very high-class instruments depends upon the number of persons who would be likely to avail themselves of the facilities thus offered. The Committee are of opinion that enquiries should be made on this point. Another allied problem should also be undertaken, viz.,

tests of the surfaces, dispersion, and definition of prisms.

"The Committee think that the above-mentioned investigations are of immediate importance, but the following subjects are suggested for investigation when opportunity allows, viz.: the transparency of optical glass, the investigation of alloys for mirrors and gratings, and the preparation and issuing of standards of colour."

Some progress has been made in carrying out this programme. The work on telescopes, binoculars, and sextants has been continued and extended at Kew, but little has been done to bring the photographic lens testing up to date, or to inaugurate the testing of high-class astronomical telescopes or of microscopes. Preparations are now being made for the work on prisms and on the transparency of materials used by Opticians, while the Photometric Laboratory, though not yet complete, is undertaking regular work.

And if I am asked why more has not been done, I must answer that other claims have appeared to the Committee more important. Our funds are, of course, limited, the requests that come for optical tests are very few, every day almost I am asked to undertake electrical, meteorological, thermometric, metallurgical, and mechanical tests, and naturally a large portion of our resources are devoted to that work for which there is most demand.

The Laboratory was established as a public institution with the object, among other purposes, of standardising and verifying instruments, and of testing materials. I am not sure that the members of the Optical Society have realised that we are willing, so far as our resources permit, to assist them in verifying their instruments or in testing their materials, and that the result of demands for such work must be that a larger portion of our resources would be devoted to the work.

However, let me turn now to what we are prepared to do, and let me say that I shall welcome discussion and criticism from the practical men whom I am now addressing as to our programme, and suggestions as to the direction in which we may extend it. The application of science to industry is the object of my work at present. I must trust you who to a large extent represent

a great industry to show me the points to which such science as we can command may be most effectively applied.

It is needless to remind you, who for so much of your work, alas! depend on Jena glass, what science has done for your industry in Germany. I have the honour to be chairman of a sub-committee of your Society, the sub-committee on standardisation in relation to optical glass. I am not going to attempt to forestall the report of that committee; it is somewhat long in coming, but the work is difficult, and experimental research is needed before we can report; still, the questions raised in the discussions of the committee have naturally influenced the direction of our work at the Laboratory, and determined the matters I wish to bring before you.

Mr. Rosenhain, who is Secretary of the Committee, at your meeting in June last read a most interesting paper on "Some Properties of Glass." He showed which of its various properties were important to Opticians. Let me take some of these and indicate how we propose to measure them. Some of the instruments we use are on the table, and it may be possible to see them in operation later.

The first property to consider is refractivity and dispersion. I take it most of you are acquainted with the ordinary method of measuring refractive indices. A prism having good plane faces and an angle probably of about 60 degs., is mounted on the table of a spectrometer, and its angle i is measured; it is then placed in a position of minimum deviation for the band or line whose index is required, and this minimum deviation, D, is found; the refractive index, μ, is then given by the formula

$$\mu = \sin. \tfrac{1}{2} (D + i) \;/\; \sin. \tfrac{1}{2} i.$$

This will, I take it, remain the standard method for such work, and though we have not an instrument such as the splendid one which the late Mr. Hilger made recently for Mr. Gifford, nor can we claim to reach the accuracy obtained by Mr. Gifford in his paper published in the Proceedings of the Royal Society, yet we do possess a good spectrometer, and can obtain results quite sufficiently exact for practical optical work of the highest character.

But this is not the method we use for commercial tests.

Some years ago Kohlrausch introduced a method of measuring refractive indices, based on the total reflexion which occurs when light passes from one medium to another less dense. The method employed in the Pulfrich refractometer is akin to this, it is very simple, and, so far as our tests have gone, is very accurate.

The specimen to be examined takes the form of a right-angled prism, A B C, fig. 1, in which C is the right-angle. The faces, A C, C B, are polished; C B must be optically flat, A C is less important. This prism rests on a rhomb of glass, D E F G, of which the faces E F, F G, are polished, the angle F being a right-angle. E F

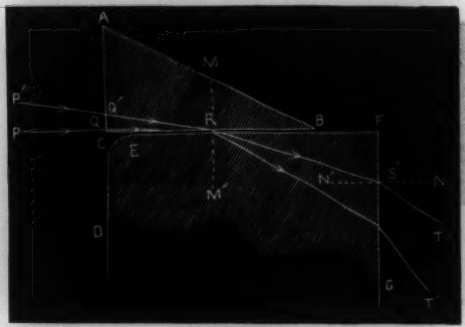


FIG. 1.

must be optically flat, and F G should also be good. The refractive indices of the lower rhomb are supposed to be known for all rays of the spectrum, and the glass of this rhomb must be more refrangible than any specimen which is to be examined in the instrument.

The prism, A C B, rests on the rhomb with C B in contact with E F, and these two faces are usually horizontal. Light falls in a direction approximately horizontal on surface A C, is refracted at the surface C B into the lower rhomb, and then refracted out through the vertical face F G. Consider a ray, P Q R S T, entering A C practically parallel to C B; it falls on this face at R at grazing incidence, and is refracted out along R S T.

No other ray can emerge in a direction making a smaller angle with the horizontal line E F than that made by S T. For, consider any other ray, such as P′ Q′ R. The angle of incidence at R is less than that for the ray P Q R; thus the angle of refraction is less also, and the ray travels in the direction R S′ T′, below R S T. Thus if an observer looked at the surface F G he would see the ray S T as the upper boundary to an illuminated space, the breadth of which depends on the convergence of the pencil incident on the face A C, and both theory and experiment show that this upper boundary is very sharp.

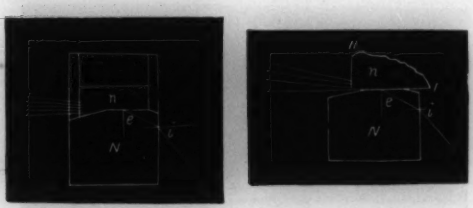


FIG. 2.

If the incident pencil consist of parallel rays coming in a horizontal direction through a collimating lens from a horizontal slit, the emergent pencil will consist of parallel rays all parallel to S T, and the observer looking through a telescope focused for infinity will see a distinct image of the slit in the direction T S.

Now, a simple relation connects this direction, the refractive index of the rhomb, D E F G, which is supposed to be known, and that of the prism A B C. For, let i be the angle of emergence at S, the angle that is between S T and N′ S N the normal at S, and let i' be the angle of refraction in the prism.

Again, let ψ' be the angle of refraction at R, the angle M′ R S between M′ R, the normal at R and R S, and let ψ be the angle of incidence. For the grazing ray, which we are considering, ψ is a right-angle. Let μ be the refractive index of the prism, μ' that of the rhomb. Then from the law of refraction

$$\text{Sin } i = \mu' \sin \quad '$$
$$\mu \sin \psi = \mu' \sin \psi'$$

and this, since ψ is 90°, becomes

$$\mu = \mu' \sin \psi'$$

Also, since F is a right-angle, it is clear that

$$\psi' + i' = 90°$$

Thus $\sin i' = \cos. \psi'$

Hence we have

$$\text{Cos. } \psi' = \frac{1}{\mu'} \sin i$$

$$\text{Sin } \psi' = \frac{\mu}{\mu'}$$

$$\text{Hence } 1 = \frac{\sin^2 i + \mu^2}{\mu'^2}$$

$$\text{and } \mu^2 = \mu'^2 - \sin^2 i$$

$$\text{Thus } \mu = (\mu'^2 - \sin^2 i)^{\frac{1}{2}}$$

Thus, when μ' is known, the value of μ is obtained by a simple calculation. In the Pulfrich instrument the rhomb usually employed has an index of 1.61511 for sodium light, and a table has been calculated, Zeitschrift-für Instrumentenkunde, February, 1888, giving the value of $\{\mu^2 - \sin^2 i\}$ for values of i from 0 to 70°. The rhomb is bevelled in front to prevent light which might fall on it just below the edge of the prism, A C B, from reaching the eye.

The instrument is adapted for use with a liquid by cementing a glass cell with a flat face on to the rhomb, as shown in fig. 2, while fig. 3 shows the arrangement

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*The above assumes the angles at C and F to be accurately right-angles. If we suppose the angle at C to be equal to $90° - \alpha$, that at F to be $90° - \beta$, where α and β are both so small that their squares may be omitted, then I find that $\mu'^2 = \text{sec.}^2\beta\ [\mu^2 \text{ sin. }^2\psi + \text{sin.}^2 i + 2\mu \text{ sin. } i \text{ sin. } \beta \text{ sin. } \psi]$, while if ϕ be the angle of incidence at Q, sin. $\phi = -\mu \cos. (\alpha + \psi)$.

If we take the case when the incident ray is parallel to C B, the incidence at R will not be strictly grazing incidence, we have $\phi = \alpha$, and the equation for μ becomes

$$\mu = \sqrt{\{\mu'^2 - \text{sin. }^2 i\}} - \beta \text{ sin. } i$$

If the angle of emergence, i, be about 45°, β should be not more than half a minute if an accuracy of 1 in the fourth decimal is required.

for raising the temperature of the liquid in the cell and investigating the effect of temperature on refractivity. In fig. 4 is shown the complete instrument, with the heating apparatus in position.

This instrument the Laboratory has, and we are prepared to make measurements of refractive indices of glass or other materials supplied in suitable form, or of liquids.

In this determination, the lines of the spectrum generally used are the yellow sodium line, and the hydrogen lines, $H\alpha$, $H\beta$, $H\gamma$. These are denoted by D, C, F, G′, respectively.

FIG. 3.

Their waves lengths are—

C	D	F	G′
656	589	486	434.

the unit being 10^{-9} of a meter.

The constants usually specified are the refractive index n_D for the sodium line, the mean dispersion $\triangle$ between the lines C and F, and the partial dispersions, C D, D F, F G′. From these are obtained the ratio $v = \frac{\mu_D - 1}{\triangle}$ and also the ratio of the partial dispersions to $\triangle$. The values are usually given for a temperature of about 20° C. The refractivities and dispersive powers of

liquids can be similarly determined for temperatures ranging to about 75° C.

Specimens of glass sent for test may be either in plates or prisms, and should have two faces at right-angles (approximately). One of these must be plane polished, and the two must meet in a sharp edge. The plane polished face may be from ¼-inch to ½-inch square. If several specimens of glass are to be sent for test simultaneously, it will be convenient if they are prepared in plates rather more than 1/10-inch thick, from six to

FIG. 4. (COMPLETE.)

eight of these plates being cemented together. The pile of plates may then be furnished with two perpendicular faces as above, these faces being also approximately perpendicular to the cemented faces of the plates. The plates may be about ½-inch square. The surfaces of the pile referred to alone need special preparation.*

*The fees are as follows:—

For a determination of the optical constants of a specimen of glass 5s. If a number of specimens be sent simultaneously for test the charge for each will be reduced.

For a determination of the refractive index of a liquid at one given temperature .. .. 5s. 0d.

For each further determination at a different temperature .. .. 2s. 6d.

It is not difficult for us in the Laboratory to fit up a vacuum tube and obtain the spectrum of the hydrogen lines. This, however, needs an induction coil, and would probably prove troublesome to an Optician who wished with the aid of a Pulfrich refractometer or some such instrument to make a series of determinations himself. I trust anyone wishing to have such a series made will write to us. Accordingly, we have been trying, though hitherto with only partial success, to discover some form of lamp which could be used on an ordinary circuit of 100 or 200 volts, and give a series of standard lines, Some form of mercury arc lamp appeared to promise the best hopes of success.

FIG. 5.

(WITHOUT HEATING APPARATUS).

Among these I may mention the Aron lamp, in which the arc passes in a vacuum between two mercury electrodes. One form of the lamp is supplied by M. Jobin, of Paris; another is the Cooper-Hewitt lamp, in which the electrodes are iron and mercury respectively. The spectrum of this last lamp shows no red, the wave lengths of the lines given by the Aron lamp are:—

Red.	Yellow.	Green.	Violet.	Violet.
615	578	546	436	406

Of course the absence of red in the Cooper-Hewitt lamp is a very serious difficulty in the way of using these lights

for our present purpose, and one for which we are trying to find a solution.

Another matter touched on by Mr. Rosenhain was the colour and transparency of glass. It is needless to urge on you the importance of this, let me illustrate by a simple experiment one insidious source of difficulty to which he alluded. The dense flint glasses have often a green or yellowish tinge, and this can be corrected and the glass made to appear white by adding a minute trace of a substance like permanganate of potash, having a pinkish colour. But this addition, though the glass appears white, markedly increases the absorption.

I have here two small glass cells, the one contains a weak solution of nickel sulphate, and is green in colour. On holding it up before a white source of light, the tint is clearly seen; on placing the second cell, containing a pink solution of cobalt chloride in front, the white colour is restored, but the light is less bright than when seen through the green liquid only.

Of course, in the experiment just described, some light is lost by the reflection at the surface of the pink liquid, and careful photometric observations would be necessary to prove completely the truth of what I have just said. If we examine the spectrum of the light transmitted by the green liquid we find it contains absorption bands in the red part of the spectrum, while for the pink liquid there are absorption bands in the green and blue.

If we wish to lose as little light as possible by absorption it behoves us to examine with care the transparency of the glass we employ for the various rays of the spectrum. Such an investigation was undertaken by Vogel, for the glasses used in his great refracting telescope at Potsdam, and the following table gives some of the results:—

Table showing the refractivity and dispersion of the glasses examined by Vogel and Müller.

		N_D	Mean Disper. C—F.	γ	Sp. Gr.
0 340	Ordinary light flint ...	1.5774	.01396	41.4	3.21
0 102	Heavy silicate flint ...	1.6489	.01919	33.8	3.87
0 93	Ordinary silicate flint	1.6245	.01745	35.8	3.68
0 203	Ordinary silicate crown	1.5175	.00877	59.0	2.54
0 598	Silicate crown	1.5152	.00879	58.6	2.59

Percentage of light of different wave lengths transmitted through 10 cm. of above glasses.

	Red	Yellow		Green	Blue	Violet		Ultra Violet	
W.L.	677	580	535	503	477	455	436	400	375
0 340	94	88	91	88	88	83	68	61	39
0 102	79	83	81	78	70	66	57	46	0
0 93	94	90	88	87	90	81	71	—	—
0 203	90	87	90	87	86	82	81	70	58
0 598	86	82	79	78	77	77	66	—	—

Just recently Dr. Kruss, of Hamburg, has published a series of important investigations on some of the Jena glasses.

Absorption in the ultra violet (Krüss).
Glasses examined.

		N_D	Mean Disper. C — F.	γ	Sp. Gr.
0 144	Boro-silicate crown ...	1.5797	.00797	64.0	2.47
0 1209	Heavy Baryta crown	1.6112	.01068	57.2	3.55
0 722	Baryta light flint ...	1.5797	.01078	53.8	3.26
0 919	Ordinary silicate flint	1.6315	.01769	35.7	3.73
0 192	Heavy silicate flint ...	1.6734	.02104	32.0	4.10

Percentage of light transmitted through a thickness of 1 *cm.*

W.L.	425	396	361	347	330	309
0 144	99	99	95	88	65	8
0 1209	97	94	65	28	7	0
0 722	96	97	85	66	32	1
0 919	96	94	66	30	5	0
0 192	91	76	16	1	0	—

The photometry of coloured lights is admittedly difficult, though Sir William Abney has shown by the introduction of the principle of flicker how the difficulty may, in many cases, be met, and the principle has been very skilfully applied by Messrs. Simmance and Abady, in their new form of photometer, recently described at the Physical Society. This we are to have an opportunity of carefully examining at the Laboratory.

But we are not now concerned with the comparison of the intensities of two lights of different colours; the question before us is whether the proportion of red to green is the same in the light after transmission through the glass as it was when recording on the glass; we need to compare throughout its length the spectrum of the transmitted with that of the incident light.

For this purpose we use a spectrophotometer, the light

from each of the two sources is formed into a spectrum by the same train of prisms and these spectra are simultaneously in the field of view of the instrument, one above the other. A diaphragm limits the field of view to a narrow strip of each spectrum, and the experiment consists in adjusting the intensity of the two strips to equality. The ratio of the two intensities is inferred from the adjustments necessary to secure this equality.

There are many forms of spectrophotometers; in some, as in Vierordt's, the adjustments are made by varying the size of one of the slits, in others the properties of polarised light are utilised. If we can arrange that one spectrum, say the upper, is polarised vertically, the other horizontally, and then observe through an analysing Nicol, then when the principal plane of the Nicol is horizontal the light from the upper spectrum will all be transmitted, that from the lower spectrum will be quenched, while, if we turn the Nicol through 90°, the reverse will be the case.

Again, let a, b, be the amplitudes of the vibrations in the upper and lower spectra respectively, and let θ be the angle the principal plane of the Nicol makes with the horizon, then the amplitudes of the vibrations in the two spectra will be $a \cos \theta$ and $b \sin \theta$ respectively, and the intensities $a^2 \cos^2\theta$ and $b^2 \sin^2 \theta$. Suppose now that we turn the Nicol until the two intensities are equal, then $a^2 \cos^2\theta = b^2 \sin^2 \theta$.

$$\text{Hence } \frac{a^2}{b^2} = \tan^2 \theta.$$

but a^2 and b^2 measure the intensities I_1 and I_2 of the two beams traversing the prisms.

$$\text{Hence } I_1/I_2 = \tan^2 \theta$$

In the apparatus now before you from the National Physical Laboratory, we have adopted the König spectrophotometer as modified by Martens. The diagram in fig. 6 will explain its action. A and B represent two narrow horizontal slits, whose length lies in the plane of the paper. The slits are of equal breadth and can be opened or closed by the same screw. L_1 L_2 are two lenses which form real images of the slits at a and b respectively. Imagine now a prism to be placed at P between the two lenses with its edge horizontal and parallel to the slits.

Each of the images, *a*, *b*, will be displaced upwards or downwards, according as the edge of the prism is below or above the paper, and will be spread out into a spectrum having its length perpendicular to the paper; if we have a diaphragm furnished with two slits in the focal plane of the lens two narrow strips of the spectrum will traverse it and reach an eye placed behind the

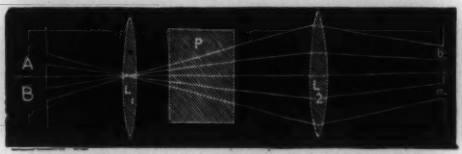

FIG. 6.

diaphragm; the diagram will still represent the path of the light for one definite wave length if we suppose it bent about a line parallel to the edge of the spectrum through an angle equal to the deviation corresponding to that wave length.

But these two patches of light will be unpolarised, and they will also be separated by a dark space corresponding to the dark space between the slits A and B.

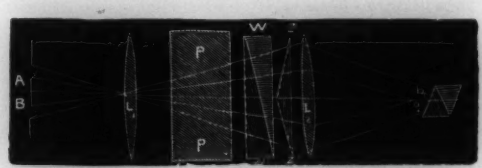

FIG. 6.*

The light can be polarised by placing in its path a double image prism W; this will give rise to two images of each of the slits, four images in all; one image of each slit is polarised in the plane of the paper, the other at right-angles to that plane, and two oppositely polarised images, one of each slit, are formed at a_o and $b\epsilon$.

On the diagram, for clearness sake, the other images a and b^o are not shown; these two images are still separated by a dark space, but by introducing, as in fig. 6*, a bi-prism, Z, into the path of the rays with its edge perpendicular to the paper, each pencil is deflected towards the axis of the system, and the two sets of rays are brought close together with merely a narrow line of division between them; we have thus obtained two spectra side by side, polarised in planes at right angles. On viewing these through a Nicol and eye lens the intensity varies as the Nicol is rotated, and the ratio of the

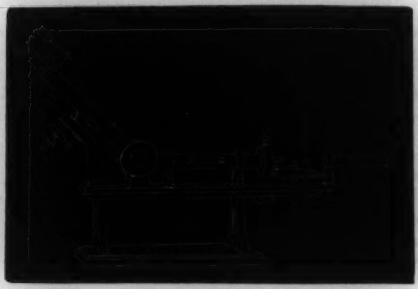

FIG. 7.

intensities in the two incident beams is, as we have already seen, measured by $\tan^2\theta$, where θ is the inclination to the horizon of the principal plane of the Nicol.

Let us suppose this done and then insert the absorbing medium into the path of one of the two incident beams, we can compare the intensity of the beam emerging from the absorbing plate with that of the other original beam, and hence find the loss by absorption and reflexion at the plate.

The loss by reflexion is eliminated by placing a second, but much thinner, plate of the same substance in the path of the other pencil. We may assume that the loss by reflexion is the same for the two plates, the difference in the losses then will measure the absorption due to the difference in the thicknesses. Fig. 7 shows the instru-

ment itself in elevation. The two beams originally come from a single slit, the light from which is divided into two pencils by means of two collimating lenses of special construction placed side by side at the distance of their focal lengths from the slit; these two pencils fall respectively on the two slits of the instrument itself. By means of this instrument we hope very shortly to be in a position to continue the work of Vogel and others in the examination of the transparency of glasses.

One question, however, naturally arises, Do not the lenses and prisms of the instrument themselves absorb light? In reply, we may point out that the total thickness of glass thus traversed is small, and that unless it absorbs just those rays which we wish to study its effects may for many purposes be neglected.

Dr. Kruss, whose special object of study was the violet end of the spectrum, used lenses of fused quartz, and prisms of quartz in his instrument; and we look forward in time to being able to do the same. Meanwhile, we hope to be able even with our present means to answer some questions of importance to industry. I have dealt with two important optical properties.

Mr. Rosenhain, in the paper already mentioned, has pointed out the desirability of tests on the durability of glass, and described how these may be carried out; we have not done any of this work yet, but there is no reason why we should not do so if there is any demand for it. I am glad to know that he is at work himself on the elaboration of a test for hardness, and hope to profit by the result of his investigations.

There are many other matters in which I am confident our tests may be of interest to you. The tests on telescopes and binoculars are well known to many of you, and I need not spend much time over describing them. They are directed to providing for the public a guarantee of the goodness of the article they are about to buy. The magnifying power, the definition, the achromatism, and other properties are tested, and a statement is issued showing if the instrument is up to standard. The instrument is not ordinarily taken to pieces, and no attempt is made to measure the optical constants of its constituent parts.

Photographic lenses are treated differently.

Kew Observatory, Richmond, Surrey.

Certificate of Examination of Photographic Objective.

CLASS A.

1. Number of objective 22974. Registered number.
2. Description Anastigmat. Diameter $1\frac{1}{4}$ inches.
3. Maker's name .....
4. Size of plate for which the objective is to be examined $8\frac{1}{2}$ inches by $6\frac{1}{2}$ inches.
5. Number of external reflecting surfaces 4.
6. Centering in mount, Excellent.
7. Visible defects such as striæ, veins, feathers, &c., None.
8. Flare spot, None.
9. Effective aperture of stops—

Number engraved on stop.	Effective aperture inches.	*f* Number.	C.I. No.*
No. 3	1.22	7.1	$\frac{1}{2}$
No. 4	1.14	7.5	$\frac{2}{3}$
No. 8	0.92	9.3	$\frac{3}{4}$
No. 16	0.63	13.5	2
No. 32	0.46	18.5	$3\frac{1}{3}$
No. 64	0.30	28.5	$8\frac{1}{2}$
No. 128	0.21	41.	17

10. Angle of cone of illumination, with largest stop = 74, giving a circular image on the plate of § 13 inches diameter.
    Angle of cone, outside which the aperture begins to be eclipsed, with stop C.I. No. $\frac{1}{2}$ = 4 giving a circular image on the plate of 0.6 inches diameter.
    Diagonal of the plate = 10.7 inches, requiring a field of $63\frac{3}{4}$.
    Stop C.I. No. 17 is the largest stop of which the whole opening can be seen from the whole of the plate.
11. Principal focal length = 8.6 inches. Back focus, or length from the principal focus to the nearest point on the surface of the lenses, = 7.8 inches.

* C.I. International Congress system.

§ The focus is for an infinitely distant object.

12. Curvature of the field, or of the principal focal surface after focusing† the plate at its centre with largest stop movement necessary to bring it into focus for an object 2 ins. from its centre 0.01 ins. forward.
    Ditto for an object 3 inches from its centre .02 inches forward.
    Ditto for an object 4 inches from its centre .05 inches forward.
    Ditto for an object 5 inches from its centre .07 inches forward.
13. Definition at the centre with the largest stop, excellent C.I. stop No. $\frac{1}{2}$ gives good definition over the whole of a 8 inch by $6\frac{1}{2}$ inch plate.
14. Distortion. Deflection or sag of the image of a straight line which, if there were no distortion, would run from corner to corner along the longest side of a $8\frac{1}{2}$ inch by $6\frac{1}{2}$ inch plate = 0.01 inch.‡
15. Achromatism. After focusing§ in the centre of the field in white light, the movement necessary to bring the plate into focus in blue light (dominant wave length 4420), = 0.00 inch. Ditto in red light (dominant wave length 6250), = 0.00 inch.
16. Astigmatism.†† Approximate diameter of disc of diffusion§ in the image of a point with C.I. stop, No. ..... at ..... inches from the centre of the plate = 0. ..... inch.
17. Illumination of the field. The figures indicate relative intensity at different parts of the plate.§

	With C.I. stop No. $\frac{1}{2}$.		With stop No. 17.
At the centre	100 :	Ditto..	100
At $2\frac{1}{2}$ inches from the centre ...	70 :	Ditto..	90
At 5 inches from the centre ...	20 :	Ditto..	60

General remarks. An excellent extra rapid wide angle lens, practically free from distortion.

Date of issue, March, 1902.

(Signed) W. Hugo, Observer.
(Signed) R. T. Glazebrook, Director.

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† Positive if movement towards the objective, negative i away from it.

‡ The sag or sagitta here given is considered positive i the curve is convex towards the centre of the plate.

§ The focus is for an infinitely distant object.

†† The objective is supposed to be perfect in other respects.

NOTE.—The following is the scale of terms used :—Excellent, good, fair, indifferent, bad.

A copy of an actual certificate will, perhaps, show best the points which are attended to, the apparatus we use is that designed some years since by Major Leonard Darwin. Our funds have not yet permitted us to order the splendid modern instrument recently described by Mr. Beck, or to instal the outfit necessary for the photographic testing of such lenses. Neither have we attacked the very difficult problem of the testing of microscope lenses.

We could make measurements of the focal length and the numerical aperture, and, perhaps, arrange some system of classification which would prove of value, but it is difficult to go far. As I have said already, hints and suggestions as to where our work may be of most use will be gladly welcomed.

Meanwhile, what I have said may, I trust, be sufficient to show that there must be close co-operation between the Society and the Laboratory.

Discussion.

DR. DRYSDALE, in opening the discussion, said he was sure that the company had testified by their applause how very much they appreciated the paper just read. The Optical Society must congratulate itself exceedingly upon the prospect of having Professor Glazebrook as President next year. The fact ought to keep the Optical Society and the National Physical Laboratory in such close touch with each other that the optical industry might depend on having the best opportunity of having its needs looked to and satisfied.

It was one thing above all others that the Society should be glad to know of, because there was no doubt in his (Dr. Drysdale's) mind, and he was sure that it could not be in the minds of many present, that the National Physical Laboratory would be of the greatest possible advantage to the optical trade. He knew of scarcely any other industry in which it was of greater importance, and he hoped very strongly—although, unfortunately, Professor Glazebrook had said that up to the present the optical industry had not made much call on the National Physical Laboratory for tests—that after having listened to the lecture that evening, many

optical manufacturers would apply to Dr. Glazebrook for assistance in their difficulties. They would, he was sure, receive very great assistance in every respect.

He hoped that, as they had got Professor Glazebrook among them, they would make a good beginning and propose some matters concerning the testing of lenses, etc., which had been mentioned in the course of the lecture. Or, perhaps, they had questions to ask and suggestions to make in which the assistance of such an institution as the National Physical Laboratory would be required.

Dr. Walmsley thought the Society must be congratulated upon having had such a paper as the one they had listened to, presented to it by such an author. It was not often that the head of a great public institution like the National Physical Laboratory came to the Society and actually begged for work. It was to be hoped that the optical trade would make a very serious note of the invitation made to them. It was a unique invitation, he thought, to emanate from such a source. Professor Glazebrook had stated that the means of the Laboratory were limited. He (Dr. Walmsley) thought that the means of such an institution ought to be practically unlimited, considering the great usefulness of its labours.

They had been told that those applicants for advice and assistance who came in great numbers were naturally attended to before those who only came in units. And surely no institution could do more than say, "Come in your thousands, or your hundreds, or your tens, and those that come in thousands will get their proportion in thousands, and those that come in hundreds will get theirs in hundreds, while those that come in tens must be content to receive theirs in units only." The optical trade must certainly take another very serious note of that point as set forth in the paper they had heard that evening.

Another point upon which the Society might congratulate itself was one which was dwelt upon in the early portion of the paper. This was that one of its sub-committees had had some influence upon the course of the work of such an important institution as the National Physical Laboratory. Professor Glazebrook had told them that the questions raised in the discus-

sion of the Optical Glass Sub-Committee of the Standards Committee of the Society had influenced the work of the National Physical Laboratory. He did not propose to discuss the scientific points gone into in the paper. They had had an exceedingly interesting description of certain optical instruments which were available at the Laboratory.

If they made their requests in sufficient numbers, and wide enough in range, other instruments would probably be provided. He thought that they certainly owed a debt of gratitude to Professor Glazebrook for the manner in which optical tests were being made at the Laboratory at the present time. The question of the standard wave lengths for specifications of the quality of glass had also been mentioned. This was one of the subjects they had been told was now under consideration at the National Physical Laboratory as well as at the meetings of the Standards Committee of the Society. He hoped that the Laboratory would soon have its funds very considerably increased.

Mr. Chalmers said he also had been very much interested in all he had heard, and especially in the work which was being done at the National Laboratory as to the properties of glass and the experiments on transparency. He also commented on the constant practice of using some other colouring matter in order to give a fictitious purity to glass. He thought this a matter of very great importance, and it would be of immense use to the optical industry in general, and especially to that part of it which dealt with photographic work.

If some standard could be made, which would give some guarantee as to the real value of the glass and of its real transparency, it would be very important and useful for photographic work, and to all those present who were engaged in any optical work. He was very pleased to have Professor Glazebrook's reference to the possibility of tests on a very extensive scale for the best type of instrument; tests, not merely as to the results that had been obtained in one particular instrument that had been made and tested, but as to the actual amount of defect that still remained in the design.

It would also be a great advantage to the optical industry, he thought, if more tests could be devised, tests which would have as their object the decision as to

whether certain manufacturing conditions, as regards the glass and curvatures, had been complied with. It would be so useful to know whether in a defective instrument the design had been carried out, either as regards curvatures or thickness, etc. Some research in that direction would be of great advantage to all optical manufacturers, and might possibly result in very great improvements being carried out in the designing of lenses.

Mr. Kendal thought that the most interesting thing about Professor Glazebrook's paper was the fact that it marked the beginning of a new era, and it indicated that at last our English scientists were doing something towards the practical application of their knowledge to the interest of the optical industry; one need not look very far back into the annals of the optical industry in this country to see that the principal factor of retrogression that had done more than anything else to bring about the sleepy condition of the manufacturing portion of the optical industry had been the sins of omission and commission of our scientists themselves, and the lack of utilitarian purpose in their work compared with that of their Continental colleagues.

The future development of the industry was still more a matter for the glass maker and the scientist; the Optician being simply limited by the available materials at his disposal. If we went back to the time of the production of the achromatic lens by an Optician, it even then was not their fault that other countries progressed at a greater rate than we did. *Our* Opticians brought the technical manipulation of the then available glasses to perfection, and it was soon manifest to both them and our scientists that any further improvement in the microscope and telescope depended on the elimination of the chromatic differences of focal length or secondary spectrum.

This necessitated the introduction of a number of optical glasses with a greater variability in their relation between mean index and dispersion. Rewards were offered by several governments and institutions at home and abroad (mostly abroad) to encourage the production of these glasses. It was a problem for the glass maker under scientific guidance. How our scientists failed in this instance and left the founding of a great industry

to the more utilitarian German professors, Abbe and Schott, was a matter of history and common knowledge.

It was evident from Professor Glazebrook's paper that this sort of thing was not past, and that our scientists were waking up to the fact that science for science's sake only was a poor ideal, and they were altering their motto to "Science for the sake of Industry." In spite of the work that had been done during the last few years to elevate the status of the Optician and general improvement of the optical trade, they had still a long way to go before we caught up to our Continental rivals. The work of the National Physical Laboratory, although a splendid beginning, must not be looked upon by any means as a climax to our efforts.

There was a latent possibility that, if they could only get a communion of effort between the Northampton Institute and the National Physical Laboratory, such a resulting influence would be exercised upon the optical trade at home that it would eventually out-rival the influence of the German Institution, the Charlottenburg Physical and Technical Institute. The aims of this institution could very well be adopted by the National Physical Laboratory. They were as follows:—

It was founded in 1887 and comprises two departments, upon which devolve the following tests:—The first department opens its doors to physical investigation and measurements, aiming at the solution of scientific problems of greater theoretical or practical importance, and involving a greater expenditure for instruments and materials, or requiring more time for observation and calculation than is ordinarily at the disposal of private individuals or even schools.

The objects of the second department are fourfold, viz., 1. To carry out physical and technical investigations ordered from time to time by the authorities or such researches as appear calculated to promote the manufacture of scientific instruments, or to contribute to the growth of allied branches of German industry.

2. To test and certify measuring and standardising instruments as far as these operations do not fall within the province of the Office of Weights and Measures; also to ascertain the errors of gradation in such instruments, and to issue certificates setting forth the results of these tests.

3. To construct instruments, or parts of instruments, and to carry out other mechanical work for the requirements of Government institutions and authorities, in cases where the work cannot appropriately be carried out in private workshops.

4. To make and supply certain parts of instruments for the use of manufacturers or traders in special cases where the manufacture in private workshops presents unusual difficulties.

These aims, in the case of our own National Physical Laboratory, should be supplemented by others. The certificates of excellence granted by the Laboratory should be made of the highest possible value by the introduction of the severest tests for the perfection of workmanship and material used in the articles examined. In the case of field glasses, a certificate, merely giving in indefinite language the magnification, the field of view, and diameter of O.G., was insufficient. There should be a statement to the effect that the glass under examination was either wholly or partially of home production. The standard of definition should be given in some concise form represented by a number of marks, the use of the term "good" or "clear" being insufficient.

A real hardship was inflicted upon the producers of the higher grade products by the use of these general terms. For instance, in his own case, they manufactured opera and field glass lenses that are scientifically calculated to give the best possible results; these are worked from costly optical glass, and the result obtained was greatly in advance of the standard obtained when lenses were worked from cheap moulds. Yet the ordinary lenses would obtain as good a certificate as the best ones under the present system, as there was no standard laid down requiring a certain result before a certain number of marks are obtained.

The National Physical Laboratory should become, he thought, a store house of information which could be gathered by the assistants testing the various instruments submitted, and tabulated for the instruction of those interested. Where manufacturers were willing to give information with regard to experiments tried with success or failure, or of material used, curves, and methods of manufacture, this should be obtained, and

would be very useful to those interested. It would prevent waste of energy that frequently goes on by the overlapping of efforts. Before spending money on useless experiments merely to accomplish something that had already been done, the manufacturer could possibly obtain, for a fee at the Laboratory, information that would prevent the waste of time in covering ground already been over by someone else.

DR. ETTLES said that he had followed the paper with the greatest interest as far as his limited comprehension permitted. Professor Glazebrook asked for suggestions, and a minor one had occurred to him which was, however, sufficiently important to justify his making it. One of his (Dr. Ettles') friends was about to start practice in a large provincial town as an ophthalmic-surgeon, and this friend had purchased a very expensive set of trial lenses. He asked Dr. Ettles to look over this set of lenses for him (there were some 240 discs in all), and he (Dr. Ettles) found that in over 30 of these lenses he could detect obvious errors. Some of them were half a diopter out and in others various differing defects were to be found. And not only that, but anybody examining the surfaces of the glass could see concentric scratches, and, moreover, many of the axes of the cylinders were incorrectly indicated.

The whole thing was a shocking example of scamped workmanship, and Dr. Ettles thought that if he, by his own rule of thumb method, could discover as many as thirty errors, had he had access to the instruments of delicate observation, such as they had had indicated that evening, he had no doubt that he would have found that very few, if any, of those lenses ought to be used. And when one considered the importance of the mistakes and wrong prescriptions caused by such faulty instruments, it was an important item for the whole community. Had he known of Professor Glazebrook's kind offer to assist struggling scientists, or pseudo-scientists like himself, he would have been most happy to have asked the Professor to look over those lenses.

A trial frame lens had certainly not got the dignity of a microscope, but he thought that, in view of the fact that they were standards under which a man must work and prescribe for errors of vision, no case of trial lenses should be placed upon the market without a certificate

from the National Physical Laboratory. He always demanded a Kew certificate with his clinical thermometers. At least, he himself would not buy another box without having it looked over by Professor Glazebrook, if he would oblige him by doing so.

Mr. Val. H. Mackinney said there were two questions he would like to ask Professor Glazebrook. The first was, Had Jena glass been officially tested and found to be the best? It was universally supposed to be so in England, but he knew for certain that one very large German optical factory preferred French glass, and if asked why, they stated at once that it was superior. It was of such importance to know where to obtain the best glass that it would be a great help if the National Physical Laboratory could inform them where to obtain it, and be prepared to test samples.

The second question was in reference to fig. 3 of Professor Glazebrook's paper (see page 62). If the refractive index was required to be taken of a liquid by the Pulfrich refractometer at different temperatures, then would not the refractive index of the rhomb of glass, which was supposed to be known, and which was represented by μ' in the formula—

$$\mu = \{\mu'^2 - \sin.^2 \dot{2}\}^{\frac{1}{2}}$$

alter as the temperature of the liquid was raised or lowered, and, therefore, give to the formula a different value?

Dr. Walmsley, again rising, said that Professor Glazebrook had pointed out how necessary it was to know whether one got the whiteness of the glass by simply adding absorbent material which lessened the transparency, or by other means. Of course, that was an important point. He had not noticed at the moment that Professor Glazebrook had mentioned a method of obtaining that whiteness by adding a substance such as permanganate of potash. But permanganate of potash, as a colouring matter, was evanescent, and, therefore, it might happen that the objectionable green or yellowish tinge corrected by such means would re-appear in process of time when probably the glass had been worked up to form part of an expensive optical instrument.

Mr. Dixey thought it was quite possible for one to

obtain a white glass which would not have the objectionable features of tinted glass on the face of it, and which would yet have the property of stopping a certain amount of light.

Mr. Ryland considered that the work of the Laboratory should be divided into two classes. In the first place, the certificate given might be very full indeed. If a certain number of marks were allowed for each defect, and the certificate made very simple, it would afford a much greater guarantee to the public than it did at present, since it would be better understood. He also suggested that a distinction be made between instruments which are to be used and carried in cases, and others, such as telescopes, etc., in which latter case he thought the certificate should be made still more simple. For example, a number of marks which could be engraved on the instrument itself might be used. Again, the optical part of an instrument could be tested, but no certificate issued unless the fittings of such instruments were satisfactory and likely to remain for some time afterwards in the condition in which they were when tested. Further, the various errors of lenses might be given as a function of the focus, so that one lens was comparable with another. The second form of certificate should be very full indeed, giving values for each error in order to provide a standard for manufacturers.

Dr. Drysdale observed that the point had already been raised in discussion during the evening by more than one speaker, as to the great necessity there was for something more absolute than had hitherto been given them in the way of tests of the errors to which optical instruments were liable. For example, to take first of all a very small point, the lack of authoritative measurements of focal length. As to the question of testing optical instruments, he thought that they wanted to have something in the way of an absolute test for the various aberrations to which lenses were subject.

They wanted to have some means of stating numerically the value of the residual aberration in each instrument, the amount of chromatic aberration, which, for rough purposes, might very often be best stated by the difference in the focal lengths. Then, again, they ought to have numerical statements of the spherical aberra-

tions, the coma, etc. He thought it was common knowledge to them all that the Kew test was not so accurate that it would differentiate decidedly between the higher class of photographic objectives. He was sure that the best modern lenses would be so far perfect that the Kew test would hardly distinguish between them.

As Professor Glazebrook had mentioned in his paper, there was the recently devised optical bench of Mr. Beck. Some such arrangement as that ought to be in the possession of the National Physical Laboratory, if nowhere else. However, photographic lenses had received comparatively satisfactory treatment. A great deal had been written on their testing, and there was no question that they had only to get certain apparatus and the tests could well be made. When it came to certain other instruments, however, matters were hardly in such a satisfactory state. The testing of microscope objectives, for instance, was in a very unsatisfactory position at present. One could hardly say that there was any standard whatever for such objectives. What was certainly required was some means of finding out the numerical specification of the various aberrations for microscope objectives.

He laid great stress on this point, for the reason that the objectives vary very considerably in their costliness. One could get an objective costing anything between £1 and £20, or even more. The fact was, that, although one knows when getting a lens, say, from Messrs. Zeiss, or the best makers, that one is going to have a good objective; still, one of such lenses is not always the most suitable for certain work which the worker might have in hand. For example, a large aperture was one of the things they all desired very much in microscopic objectives, but where there was a case in which the objects were very much in relief, a wide aperture was quite unsuitable. If they could have a specification of the various errors for microscope objectives it would be easy for everybody to ascertain whether the objective was particularly suited or not to the purposes for which they required it, and also how far it was necessary to go.

As to the question of telescopes, there was nothing to be said concerning the larger astronomical instruments. Where, however, tests were very much to be desired was

in the case of the smaller instruments such as the popular prism binoculars. Certainly tests were demanded which would be more searching than for the ordinary Galilean field glass. Among the things of importance, he thought, they most certainly ought to know something about the perfection of the collimation of the telescopes. It had been pointed out that very serious eye-strain was caused if the axes were out of adjustment. Aperture was also an important point. In Galilean glasses it was a secondary consideration, but in the prism it was of very considerable importance. If the object glass of any telescope bore the same ratio to the pupil of the eye as the magnification, then that telescope, apart from absorption in the media, should give the same illumination of field as the field viewed itself.

In other words, if, for example, they had a glass which gave a magnification of 12 diameters, then if that glass had an object glass of sixty millimeters in diameter, taking the maximum pupillary diameter as 5 mm., and there was no absorption in the intervening lenses, the illumination of the field should be precisely the same through the glass as without the glass, and they could not get a higher illumination than that. That should be taken as the highest standard, which they could not hope to reach. However, the illuminating powers of all glasses should be expressed in percentages of this ideal. Professor Glazebrook had explained to them the Pulfrich refractometer. He, himself, had not had an opportunity of using it, but he had had a great amount of experience with the other instrument made by Messrs. Zeiss, the spectrometer, which was specially used for glass testing.

That spectrometer had very many advantages over the ordinary form of spectroscope. In fact, he was not sure it was not quite as quick as the Pulfrich type, but it did not come very far behind it, and its great advantage was in the fact that they had not to depend on the refractive index of their rhomb of glass, or to trouble about the question of temperature of the rhomb. He, personally, was inclined to prefer an absolute instrument rather than a relative one, and he should like to know whether Professor Glazebrook had had any experience with the other instrument, and whether he found the

Pulfrich the better of the two. In his opinion, the great trouble of glass testing was the limitations existing with the usual light, and he was very glad to see that this subject had engaged the attention of the National Physical Laboratory.

Dr. Drysdale then called upon Professor Glazebrook to reply.

Professor Glazebrook expressed his extreme gratitude for the way in which his paper had been received, and also for the valuable information that had been given him in the discussion that had arisen. He would lay to heart much that had been said and hoped to have further opportunities of discussing some of the points mentioned with the members. He had been very much interested in what Dr. Chalmers had said as regards the importance of the transparency required for photographic lenses. One point he had been considering was as to whether, in a spectro-photometer, it was necessary to go to the degree of refinement that Dr. Krüss had.

He had not quite satisfied himself whether it might be possible to construct an instrument out of glass which would show so little selective absorption that results obtained by that instrument would really be of value for photographic lenses. Mr. Rosenhain was of opinion that it could be done. With regard to what fell from Mr. Kendal, he (Dr. Glazebrook) knew the catalogue of the Paris Exhibition extremely well, and had rarely spoken on a subject such as that under discussion without alluding to it, and to the progress it showed in German work. However, he did not think they need spend time that evening in endeavouring to decide whether the blame was to be laid at the door of the Optician or the mathematician.

He dared say that both might be somewhat at fault No doubt mathematicians had failed sometimes to interest themselves sufficiently in the practical work out side their theories, but at the same time it was of very great value and importance to the practical man to have the theories. He thought, however, that the practical man—the Optician, the mechanic, etc.—had not sufficiently realised even yet in England, certainly not so much as they had done in Germany, the commercial value of science as a factor in the progress of their own

industry. He felt confident that if the workman would rally, and in the proper spirit, apply to the scientific man for help, and show that he truly wanted that help, he would immediately get a great part of it.

At any rate, he could assure Mr. Kendal that that was the sole object of the existence of the National Physical Laboratory. If Mr. Kendal would help them in pressing on the Government the need for extending their opportunities for work in regard to funds and so on, they would be most glad to devote any scientific ability they possessed, or any section of their abilities, to the optical problems that Mr. Kendal had brought before them. With regard to one or two of the minor questions, they would be equally glad to take up at once the testing of trial lenses as desired by Dr. Ettles. He was not certain that they had everything which was necessary at the Laboratory at the present moment, but he thought they could put that work in hand quite easily and quite thoroughly.

With regard to the relative value of glass, referred to by Mr. Mackinney, in speaking of the glass manufactured by the French and the Jena firms, he (Professor Glazebrook) knew that the former were making extremely valuable glass, and he dared say they could find many other firms making glass equally as good, and, perhaps, better than the Jena glass. He hoped that they might look forward before very long to having English glass, or some of the most important glass, made in England. Referring to the remarks of the President, Dr. Glazebrook said that with regard to the need for more absolute measurement in certain lenses, he would like to remind him that in the Kew certificate the various aberrations which he had referred to were all mentioned. He understood, however, that what was asked for was that the corrections should be given to a higher order of exactness.

With regard to telescopic lenses, he took it that the instrument should be treated as a whole, but also that a certain definite statement as to performance was desirable, and not merely a certificate that the instrument came up to a certain standard. At Kew, the telescope tests were arranged, in the first instance, with regard to the requirements of the Navy. A telescope was selected and tested very carefully, and very completely,

indeed. That telescope was selected as a pattern, and, then, all that is asked is that the other telescopes should come up to that pattern. This plan, Dr. Glazebrook thought, might have had as a consequence the method of finding out whether the instrument simply came up to the pattern or not, and did not show sufficiently what the defects were if it was rejected, or the points of excellence if it passed the standard. It would be of more interest and importance to those present if these points were emphasised more fully.

He thought that marks might possibly be attached to the instrument as they were given to watches. This would lead also to stimulation among manufacturers. Dr. Glazebrook quoted the case of the pleasure caused in foreign horological circles by the fact that a watch of Swiss manufacture had gained the highest number of marks during the past year. He hoped to have other opportunities of discussing with the members of the Society who were interested in the work, the manner in which some system of that kind could be best introduced, and the points to which it was important that attention should be called in drawing up any form of certificate. With reference to the temperature in the Pulfrich instrument, it did not very much affect the refractive index of the glass as it did of the liquid. Of course, in using high temperature liquids on the glass, a certain amount of care had to be taken. In making the observations, it was best to make the whole of the instrument, as far as possible, of the same temperature.

THE TESTING OF PHOTOGRAPHIC LENSES.

PAPER BY S. D. CHALMERS, M.A., *Member.*

Read at the Optical Society on February 18th, 1904.

IN a paper read before your Society some time ago your worthy President deplored the absence of any complete accessible account of the various methods of testing lenses; his paper did a good deal to supply this deficiency, but the extent of the subject made it necessary to omit many details which might be interesting to you. This has induced me to give an account of the various methods in use for the testing of photographic lenses, hoping that with a smaller subject it might be possible to refer to matters of detail. With the same idea I

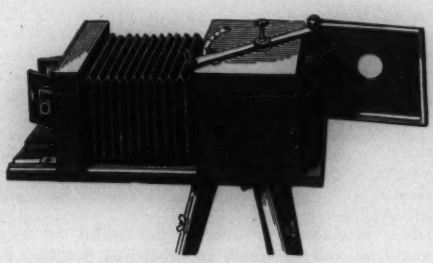

FIG. 1.—THE TOURNIQUET—GENERAL VIEW.

have decided to omit all reference to the testing of defects due to colour, hoping at some future time to be able to deal with that subject in detail.

This paper does not profess to do more than describe the various methods and offer some criticism as to the advantages and limitations of each, suggesting some directions in which progress may be looked for.

The object of a photographic lens is to produce on a flat plate an image of an extended object, every point of which should be well reproduced at the corresponding point of the image plane, and, in general, it is desirable that this image shall be as bright as possible, consistent with the above requirement.

Parts of the object may, however, be at different distances from the lens, and it will be found impossible to form quite sharp images of the points which are in different planes, except by reducing the aperture, and thus diminishing the brightness of the image. But though it is impossible to obtain good images of points at different distances in the one photograph, it would be possible, by adjusting the position of the plate, to obtain in different photographs sharp images of points at different distances. Still, it will be found that there is one distance for which any lens most nearly satisfies ideal conditions.

Photographic lenses may be divided into two classes: those which are designed to give the best corrections for infinitely distant points—the correction being good for any point at a considerable distance from the lens—and those which are designed to give the best results when used at shorter distances—corrected for a distance about three or four times the focal length. In the one case the various points of the object may be regarded as at the same distance from the lens, while in the other they should lie in a plane at right angles to the axis of the lens.

These two classes of objectives require rather different treatment, and it will be found that certain methods and apparatus which are well adapted to one do not apply equally well to the other.

Before proceeding to describe the testing of lenses it would be well to recall, as briefly as possible, the nature of the defects from which the lens system might suffer, as well as the constants which it is desired to measure. In the ideal lens all light proceeding from an object point would pass accurately through one point on the plane of the image.

In the case of a point on the axis (or with light parallel to the axis), the light which is incident on the various zones will in general come to a series of foci on the axis, and the appearance on the image plane will be a number of concentric circles of varying brightness, the defect being known as spherical aberration; when the foci of the central and extreme outside zones coincide this reduces to the less serious defect, zonal aberration.

Again, the magnifications produced by the various zones may not be the same, and hence the image of a

point outside the axis will suffer from the defect of coma. This defect is one which it is extremely difficult to express numerically, but which is very important in lenses of large aperture—fortunately, it is a defect which is almost eliminated in symmetrical lenses of apertures up to about F. 7, the residual amount being practically determined by the correction of astigmatism and the aperture ratio.

For a point some distance from the axis we have the defect known as astigmatism—the pencil of rays passing approximately through two focal lines, except when there is coma or spherical aberration of a serious kind; in this case they can hardly be called lines. It is usual to determine, by observation or calculation, the position

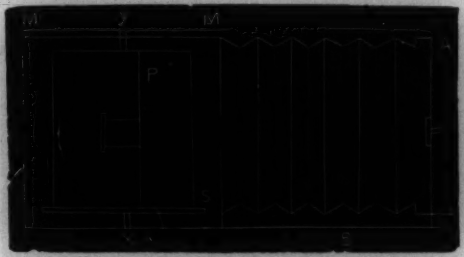

FIG. 2.—THE TOURNIQUET.

of the smallest spot of light, and to consider this as the image position; the various points so found lie on a surface which forms approximately portion of a sphere whose curvature is easily obtained.

But even if there be a perfect image it does not follow that it is in the position corresponding to the object point; the magnification may be increased or decreased towards to the outer part of the field, and give rise to the defect known as distortion. It is necessary to be able to measure these various defects and to estimate the loss of light due to the absorption by the lens or reflection at its surfaces. In addition, we require the constants of the lens, its equivalent focal length, the aperture and the field of view throughout which the

image receives light from all parts of the effective aperture.

As the latter is only wanted approximately, it is sufficient to look at the lens from points in the image plane till the light begins to be cut off by the lens mount, and so estimate the field of view. The various tests for effective aperture have been given by Dr. Drysdale, and the various methods used to obtain the equivalent focal length are given in describing the different systems of testing.

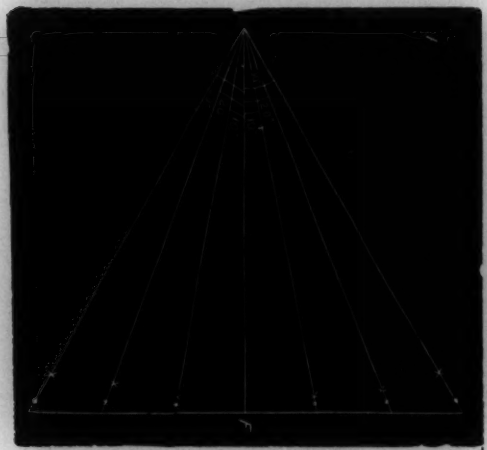

FIG. 3.—TORNIQUET DIAGRAM.

The methods of testing for defects may be divided into three, or, rather, four groups, according to the facts which are determined in each case. The first and most obvious method of testing is to obtain an image of small sources of light placed in the positions which the objects photographed would generally occupy, and to examine these images either directly or allow them to be impressed on a photographic plate and subject it to examination.

This method is specially suitable for obtaining a general idea of the performance of a lens, but it has the great defect that it does not easily furnish a numerical value of the defects in such a way as to make the test independent of the observer's peculiarities. It is true that the size and shape of the image can be determined, but this alone does not necessarily define the performance, as a small definite spot surrounded by a larger faint fringe is not necessarily worse than a definite evenly illuminated spot of intermediate dimensions. And it has the further disadvantage that it gives no information which would help in the design of a lens, or in obtaining in practice the best results with one already made.

The second method aims at determining the position

FIG. 4.—THE BECK-LENS TESTING BENCH.

By permission of Messrs. R. and J. Beck.

at which the best image is formed of each of the object points. In this way it enables us to obtain a numerical estimate of the defects of astigmatism and curvature of field, but it is difficult to estimate the defect of coma and the value of residual spherical aberration is only determinable approximately, while the accurate determination of the position of focal lines is not easy when the astigmatism is accompanied by other defects.

The third method, which is practically identical with the above, aims at determining the positions of the object points which give the sharpest images on the plane of the photographic plate. Its defects and advantages are in principle much the same as those of the above, but it has the merit of being specially adapted

to photographic tests, and with the usual test the imperfections of the observing instrument—a telescope—are less apparent.

The fourth method is to determine the paths of the various rays through the system and thus to determine the positions in which they intersect the plate. This method is extremely valuable from the point of view of the design of lenses, and has the great merit that it can be applied to an uncorrected, quite as well as to a corrected lens. Its defect in principle is that it only gives the performance of the lens indirectly, but the accuracy and detail are very great.

It now remains to see the methods based on these various principles as they have been actually used, but as most systems of testing make use of more than one of these principles it will be convenient to describe a number of the most common systems.

THE TOURNIQUET.—In this instrument the second principle is almost exclusively applied. The apparatus is shown in the two diagrams; it consists of a hollow cubical box (with openings at the front and back) which is connected by a camera bellows to the carrier A; in the cubical box is mounted a smaller box, with two sides removed, which is capable of a complete revolution about the axis $y\ y_1$; in this box is a panel P—to carry the lens to be tested—which can be moved forward and backward by the screw S.

The carrier can be racked along the baseboard B, and carries a vernier which determines its distance from the axis $y\ y_1$. In the middle of this carrier are two small hinged panels, one carrying a small framed ground glass, the other a micrometer scale, which can be read by a magnifier; either panel may be brought into position and the image observed on the ground glass or measured against the scale—the latter being capable of rotation, to measure lengths in all directions. The instrument is mounted on a substantial base and is provided with a pointer $M\ M_1$, to measure the rotation of the lens about the axis, and a sighting arrangement to set the axis of the lens in the direction of the small source of light; the latter may be a distant object or an artificial star.

To measure the focal length of a lens, it is mounted in the panel and the image is brought on to the ground

glass, the lens is rotated about the axis; if the image be stationary the focal length can be read off on the vernier. If the image moves, it is necessary to adjust the lens relatively to the axis till there is no motion of the image. For more accurate measurement the position of

FIG. 5.—DIAGRAM SHOWING PRINCIPLE AND EXTRA-FOCAL METHOD.

the image can be measured on the micrometer scale and any movement readily detected.

To measure the spherical aberration the diameter of the image of the point of light can be measured, or the focusing points of various zones of the objective can be determined.

To determine oblique aberration the lens is rotated through any selected angle about the axis $y\ y_1$, which has been made to coincide with the back nodal point, and the image can be received on the ground glass; if the position of the carrier be adjusted till the image appears successively as a fine vertical line and a fine horizontal line, the distance of the two focal lines from the nodal point are determined, and by the aid of the diagram the curves for the focal surfaces may be drawn. If, in addition to this the shape and size of the smallest disc of light be measured some estimate can be formed of the amount of coma present.

To measure the distortion it is necessary to measure the amount of motion of the image when the lens is rotated through a considerable angle. From this the position of the image may be obtained graphically or by calculation. This instrument might be used simply to determine the size of the image at all points on the image plane, but this would involve much calculation and nice adjustments. Since the source is always at the same distance it is evident that the instrument is unsuited to the testing of process lenses which should be corrected for a flat object with the various points at different distances from the lens.

THE KEW TESTS, AS USED BY THE NATIONAL PHYSICAL LABORATORY.—In the apparatus designed by Major Darwin many of the advantages of the tourniquet have been retained, and the measurements are in general conducted on the same principle. But it is interesting to notice that two tests of defining power have been included. For central definition, a thin, straight strip of steel 1/10 inch wide—capable of rotating about the direction of its length, to make it appear, when seen against a bright background, as a thin black line of varying thickness—is employed, and the defining power is measured by the smallest apparent width which can be distinctly seen.

The definition throughout the field is tested with a similar object which is fixed to have a definite apparent breadth, the stop being determined which permits of this being distinctly seen. A test of the transparency of the lens for visual rays is in use, but, unfortunately, this gives little information as to the transmission of much of the light which would be valuable in photography.

THE BECK BENCH.—While retaining the advantages of the system of rotating about the nodal point, Messrs. Beck have devised a bench which also permits of a satisfactory test of process lenses. The apparatus consists of a divided bar which carries a number of fittings; one of these carries an axis about which the platform L rotates; on this is another platform which carries the lens holder and is capable of motion by means of a rack and pinion R.

The lens holder consists of a series of screwed rings to take lenses of various sizes. A cross bar slides on the main bar and carries a fitting on which is mounted a microscope to be used for the examination of the aerial image. Thus the microscope can be used—as in the tourniquet—in a fixed position, or can be travelled along the cross bar to examine the images of points not on the axis of the bar. This motion permits of the testing of process lenses under favourable conditions.

But in the case of ordinary lenses it seems to me desirable to reverse the procedure, and, instead of employing a collimator and examining the image by a microscope, to employ a telescope of long focus to examine the image of a fine test object placed in what is usually the image position. The advantages seem to be the greater perfection of the telescope as an observing instrument, and the fact that the errors to be measured are increased in the ratio of the squares of the focal lengths, thus reducing the error in measuring the position of the focus. To obtain the full advantages of this method the object must be arranged to remain always on a plane at right angles to the axis of the lens, and the measurements made by the change in focus of the telescope. The defining power of the combination, when the telescope is focused for infinity, might be made to furnish a simple, yet effective test of the whole performance.

As an additional test, and especially for process lenses, Messrs. Beck employ a test object and examine its image in the position where the photographic image would be received on the plate.

Of the methods based on the first three principles, it remains to refer to the photographic tests, a good example of which is furnished by Rudolph's method; it consists of a test by the first method, *i.e.*, a photograph

of a geometrical design which is placed in a plane at right angles to the axis of the lens, the central portion being focussed and the resulting photograph examined for definition; the remaining portion is a photograph of a number of series of cards, each set being arranged in a spiral with its axis parallel to the axis of the lens, the test being which member of each group is most sharply defined.

In the method used by Dr. Harting, the last object consists of a series of geometrical designs, which is focussed as sharply as possible, and a series of photo-

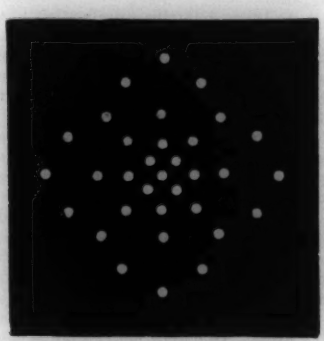

FIG. 6.—A PHOTOGRAPH BY PROFESSOR HARTMANN. TELESCOPE OBJECT GLASS, TESTING ZONAL ABERRATIONS.

graphs taken, one in this position and others with the plate in slightly different positions inside and outside the focus.

Such methods are satisfactory in giving an estimate of the general performance of a lens, but are not in general sufficiently accurate to give numerical measures for the various errors. But the great defect of all these methods is that the measures are to a very great extent subjective, and are all, more or less, affected by the personal element in the choice of the best focus. Dr. Harting's method does, to some extent, remedy this, but

it gives practically no numerical estimate as to the performance of the lens.

The fourth principle can, however, be used as the basis of a method which seems to leave no room for possible personal variations, and in addition to this it would seem to be the most desirable from the point of view of the results obtained and the information it furnishes; but at first sight it seems practically impossible of realisation; when sufficiently small pencils of rays are taken to be regarded as approximating to the ideal single ray the intensity of any image formed would be too small to permit of its being observed or focussed for photography.

Even if two small symmetrically placed openings are used, it is practically impossible to determine the point

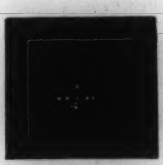

FIG. 7.—TWO EXTRA-FOCAL PICTURES. ANOTHER STOP.

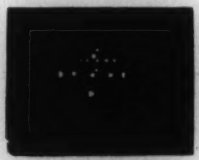

FIG. 8.—THREE EXTRA-FOCAL PICTURES. DIFFERENT POSITIONS OF PLATE. SAME STOP.

at which the two images can be brought into exact coincidence. But Professor Hartmann has given a most ingenious method of determining the foci of various small portions of a lens. By the use of photography he has avoided the principal difficulties associated with the small amount of light admitted by very small openings in the plane of the stop. By taking two photographs—one well inside and one well outside the focal plane—he has avoided the necessity of focussing, and with it the possibility of personal error.

Before the lens—for central aberrations—or at the plane of the stop—for oblique aberrations—are placed two small symmetrically situated circular apertures. When a small source of light is photographed we obtain on each plate a reproduction of the stop on different

scales. From the diagram it will be seen that light proceeding from the object point to the point A on the stop will, after passing through the lens, cut the planes P_1 P_2—where the photographs are taken—in the points a_1 a_2; similarly light passing through B C D will meet these planes in b_1 c_1 d_1 and b_2 c_2 d_2 respectively, and thus the pairs of points a_1 a_2, b_1 b_2, c_1 c_2, and d_1 d_2, each determine a definite ray passing through the points A B C D respectively.

If, now, l be the distance between the planes P_1 and P_2 the intersection of the rays d_1 a_2 and d_1 d_2, is at a distance from $P_1 = l \times \frac{a_1 d_1}{a_1 d_1 + a_2 d_2}$. If h_1 be the height of a_1 and h_2, the height of a_2, from the axis, the inclination of the ray a_1 a_2 is given by $\tan u = \frac{h_1 + h_2}{l}$

Hence, we obtain both the position of the focus and the equivalent focal length for the point A, from these two photographs, and by the use of a number of pairs of apertures this pair of photographs may be made to determine the whole course of the correction for spherical aberration and for coma.

For the testing of astigmatism and oblique aberrations it is only necessary that the object should be in a direction suitably inclined to the axis of the lens; in the case of process lenses the object should remain in one plane perpendicular to the axis of the lens—the plane for which the lens is to be tested. For oblique aberrations generally, it is desirable that the stops used should occupy the position of the effective stop of the system, the apertures should be a series of small openings grouped in two circles—radius of one being 2/3 that of the other. A small central hole will serve to determine the distortion.

The practical difficulty is the securing of a sufficiently bright, small source of light which should be practically monochromatic, preferably green or blue. There are two methods which may be used, the best being to employ a collimator of large focal length, at the focus of which is a small opening illuminated by light condensed from an arc lamp, preferably with metal electrodes. This has the defect that any errors of the collimator will affect

the results obtained, and any error in determining its focus will affect the absolute value of the focal length determined. But when the focal length of the collimator is a number of times greater than that of the lens to be tested, the effects of its errors being reduced in the ratio of the squares of the focal lengths, these can be neglected.

The other method is to use an arc lamp at a finite distance with a small aperture in front of it; this has the defect that the photographs only enable us to find the position of the conjugate point directly, but the focal lengths and position of the principal foci can be calculated.

In either case the size of the object will prevent the exact coincidence of the points a_1 b_1, etc., with their theoretical positions, but if the difference is sufficiently great to be appreciable with the source of light used,

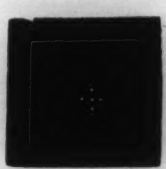

FIG. 9.—TWO EXTRA-FOCAL DIFFERENT POSITIONS, SAME STOP.

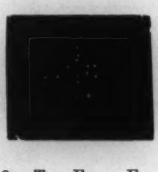

FIG. 10.—TWO EXTRA-FOCAL, PICTURES OF ONE STOP.

the effect can be eliminated by considering the mean of the values for a_1 and b_1, and taking this to correspond to the mean of the values of A and B and to that of a_2 and b_2.

The accuracy of the method is limited only by the accuracy of the measurement of the distance between the two positions of the photographic plate and the accuracy of the measurements of the photographs and stops; the error in estimating the focal length being practically the error of the latter measurement, multiplied by the F number corresponding to the circular aperture which would just include the small portions of the lens considered.

It will be noticed that the accuracy of estimating the differences of focal lengths of the different portions of

the objective is very great, the error in a lens of 250 mm. focus should not exceed .1 mm. Unfortunately, want of time prevents my referring to the application of this method to the determination of chromatic aberrations for which it is singularly well adapted.

One advantage of this method for the purpose of a thorough going test is that the actual measurements are such that they cannot well be influenced by personal considerations, since the observer can hardly know the meaning of the measurement without calculation; this constitutes a defect from the point of view of approximate rapid testing.

In conclusion, I would commend to the Society's notice the necessity for arriving at some standard by means of which the excellence of lenses belonging to different classes, or meeting slightly different requirements, might

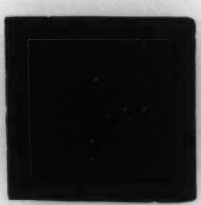

FIG. 11.—TWO EXTRA-FOCAL PICTURES.

be fairly compared; and, also, call attention to the desirability of devising tests which might be useful in locating errors which have arisen by departing from the design, either as regards the glasses employed or the curvatures and thicknesses of the lenses.

One such test I can suggest for such lenses as satisfy the Petzval condition. I have shown that this condition requires that the two focal lines should be at distances from the focal plane which are in the ratio of three to one. As this condition involves only the refractive indices and the curvatures, advantage might be taken of this fact to determine whether the error lies in the thicknesses or not. In making the measurement it is desirable to choose the portion of the field mid-way between the axis and the outside.

I throw this out as a suggestion in the hope that it may induce some of you to devote special attention to devising better tests with this end in view.

Discussion.

THE PRESIDENT, in opening the discussion, said that Mr. Chalmers' subject was one which must be of the greatest importance to all present. The author had presented it to them in a very interesting manner.

DR. GLAZEBROOK said that the Society was to be congratulated very much on the paper they had listened to. It had been extremely intersting, and would, he was sure, prove of great value. He himself was not thoroughly acquainted with the method of testing lenses that had been described. It seemed to him to be a method requiring very full and very careful investigation, but likely to lead to valuable results. Mr. Chalmers had referred once or twice to the Kew tests, and suggested that he (Dr. Glazebrook) might give some information with regard to them. There were one or two points to which he should like to call attention, if he might be permitted to use the blackboard. He should also like to refer to another method of making tests photographically, which had not been referred to by Mr. Chalmers, but it seemed to him to be worth consideration.

As Mr. Chalmers had said, the principle of Major Darwin's testing apparatus was a modification of the tourniquet method, but it possessed one or two advantages over that method.

[Dr. Glazebrook here illustrated the Kew method on the blackboard by means of diagrams.]

The Kew method, he said, consisted mainly in the fact that there was fixed, in the Kew tourniquet, to the frame carrying the lens a plate at right angles to the lens which moved with the lens. One of the advantages their system seemed to possess was that it was quite easy, by means of a series of measurements, to obtain information as to the focal length of the lens, and other qualities. It was not necessary to measure the distance from the axis of rotation.

With regard to the test of definition, Mr. Chalmers described Major Darwin's test, but that had been found somewhat difficult to carry out, and had not been used. The test at present used at the Laboratory, consisted of

simple observation of two fine lines at right angles, as shown herewith The lens was focused for these lines, and then the whole instrument was turned until the image of these fine lines fell somewhere about half way between the axial position and the extreme edge of the plate. The focus was examined again in that position, and the lens was re-focussed, a wide angle stop being employed. The camera is then turned until the image of the lines comes to the extreme edge of the plate, and the lens is stopped down until the image of the lines is again distinct. With regard to the method of Professor Hartmann, one of the points which struck him was the great length of time necessary to test the lens, so as to obtain, say, the errors of curvature and astigmatism. With the Beck bench, these errors could be obtained in a very few moments.

In a pamphlet he had with him, there was described another method of testing lenses, in which a photographic image of a specially planned object was obtained. He (Dr. Glazebrook) had not seen any allusion to the method in English papers. It was published by Mr. Houdaille in 1894, and the method was what Mr. Chalmers had called the "inverse method" of testing a lens such as the process lens. At Kew, at the present moment, there is practically no method of testing process lenses.

[Dr. Glazebrook here again had recourse to the blackboard to describe the method mentioned.]

The point to which he desired to draw special attention with reference to this method of Houdaille was that the experimenter endeavoured to obtain all information about the lens from the photograph of a series of scales arranged in a particular manner, so as to be at slightly different distances from the lens, and inclined at various angles to the vertical. These scales carry as test objects a series of groups of circular dots. In each group there are seven dots, six being at the angles of a regular hexagon, and one in the centre. The dots are $\frac{1}{2}$ millimetre in diameter, and their centres are $1\frac{1}{2}$ millimetres apart, so that there is $\frac{1}{2}$ a millimetre between any two adjacent dots. The bench is arranged to work with 1/5 reduction, so that the dots appear as of 1/10 mm.

on the photograph, and the spaces between them are 1/10 mm. wide.

A lens is defined as having a one-tenth defining power in any direction if it completely resolves such a series of dots. When the photograph is taken, the groups are found to be resolved over a definite area of the plate, and from the size and shape of this area, Mr. Houdaille shows how to determine the properties of the lens. From a single photograph much valuable information is obtained. In conclusion, Dr. Glazebrook again expressed his high appreciation of the value of the paper, and the interest of the method of testing employed.

Mr. Conrad Beck expressed his pleasure at having been privileged to listen to Mr. Chalmers' paper. Professor Hartmann's method was new to him, consequently he was unable to make any remarks upon it, except that it showed promise of being a very beautiful test. He proposed to confine himself to certain tests mentioned in the paper, of which he had had practical experience. The method of measuring the spherical aberrations of the lens, by means of measuring the diameter of the image by a point of light, proved to be in practice of little value, as it was almost impossible to disassociate from the appearance caused by spherical aberration that which was caused by diffraction. Also in certain cases where a small zone of the lens had a different focus to the rest, a general fine haze of light was thrown over the image, whose diameter it was almost impossible to measure accurately. The method of measuring astigmatism by focussing to the focal lines, in practice became equally unsatisfactory.

The so-called focal lines were seldom lines, but were either the figure of 8, or some irregular shape dependent upon coma, etc., the focus of which was difficult to determine. The focussing to gratings, provided a microscope were used, was a perfectly reliable and accurate test of astigmatism. One of the methods adopted of the Beck Lens Testing Bench depended upon the use of a high-power microscope for examining the image. The difficulty of ascertaining the exact position of a focus was very great, if only low magnifying powers were used, owing to the accommodation of the eye. The use of a high-power microscope overcomes this difficulty. In certain cases a ground-glass may be introduced in the focus

of the microscope, as by this means the image became a luminous object, and a wider angle object-glass could be used in the microscope, thus still further avoiding the difficulty experienced in ascertaining the exact position of the focus. But, owing to the ground-glass destroying the definition of the image it could only occasionally be used, and experience showed that a fine layer of wax formed a better screen than ground-glass for this purpose.

A further advantage in the use of the microscope was that monochromatic solutions could be used in the microscope itself, without introducing any error such as would be the case if they were introduced between the photographic lens and its image. He was very much interested in what Dr. Glazebrook had said with reference to the Kew definition test, as his own experience had thoroughly confirmed what he had said on that matter. It was extremely difficult to decide at what position a line was or was not visible, whereas, with gratings or lines that could be divided, it was quite possible with a sufficiently powerful microscope to obtain a distinct opinion on this point.

With reference to the question as to whether visual or photographic tests were the best, the photographic test was invaluable as a check, but he considered that for all lenses for dry-plate photography photographic tests could be replaced by visual tests with proper monochromatic solutions, thereby enabling much better numerical and quantitative data to be obtained. Lenses which were to be used for distant objects could not be satisfactorily tested photographically without unusual opportunities for arranging a series of objects to be photographed at a great distance from the camera. Consequently, most photographic tests resolved themselves into apparatus placed comparatively near to the lens, and were liable to give a totally erroneous idea as to its capabilities for distant work. One department of lenses, namely, those used for process work, required photographic testing, as they are entirely used upon wet plates, which require blue, violet and ultra violet light to be corrected, and it would be a great advantage to Opticians if their chemical friends could produce a fluorescent screen which would render these rays visible.

A further extremely interesting point was how the tests

on photographic lenses should be expressed, and this point required careful discussion. Again, it was not fair to give all classes of lenses the same test, as many lenses were manufactured specially for one purpose. For example, a process lens, to be used with wet plates, should not be submitted to the same test as a lens designed for snap-shot photography or portraiture on dry plates, and a series of different tests was required to truly demonstrate their efficiency for the particular purpose to which they were to be put. The test by means of a collimator, which was suggested by Mr. Chalmers, looked as though it would have very many points in its favour, but without practical experience it would not be safe to predict too much for it.

Mr. Blakesley expressed his approbation of the paper. He agreed with Mr. Beck in thinking that somewhat simpler methods than photographic methods should, if possible, be adopted in the testing of photographic lenses. He himself had gone into some of the matters connected with lenses, with a view to accurate testing, and his plan had been to obtain quick results if possible. That would be of great advantage to anybody who had to deal with a large number of lenses. He had lately published a method by which the focal length of a lens could be read off on a scale; in this, he had gone into the subject of lenses generally. He described one of his methods on the blackboard by means of sketches, a method which he had found to be very useful for practically any lens.

Mr. Horace Beck said there was one point about Professor Hartmann's method which seemed to be extremely useful, and that was in cases where lenses were very much out of correction. In many cases, when the spherical aberration in particular was bad, it was extremely difficult to get accurate tests. This method, however, seemed to him to be almost as efficacious in measuring large errors as in measuring lenses which were almost accurate.

Mr. Selby had also been particularly interested in the method described. The objection which had been raised more than once seemed to be the length of time it required. When the system of lens testing was first introduced, one of the considerations taken into account was the expense, and also the time. Photographic

methods were at that time ruled out for this very reason. No doubt, at the present day, that did not hold with quite the same stringency. The lenses made now were made with such accuracy that more extensive and more accurate tests were required, and, consequently, more exact instruments were necessary. But they would all like, before testing, to know something of what a lens was capable of doing, and to have an exact statement about its possibilities. The Kew tests had been of great service to them, having enabled them to obtain an idea of the general purposes of the lens, even though they had scarcely given the lens-maker sufficient information to supply him with all he desires to know in regard to different aberrations.

Mr. Bull remarked that with reference to the requirements of process work, three-fourths of the light recorded on the wet plate was invisible to the human eye. He thought that a test which was not photographic would be useless unless the lens was going to be used for either orthochromatic or three-colour photography.

Mr. Purser desired that a comparison of lenses should be prepared for publication, as it would be of very great value to all. It should be made public that, though curvature of the field could be avoided, it might, in very many cases, be of great advantage to the lens.

Dr. Drysdale thought that there were two points which wanted to be brought out in the discussion. The first was that they required to know the best methods of testing lenses. The second was the very important matter of the expression of the results when they had been obtained. This, he thought, was very important, because the discussion of different methods of testing must, to a large extent, depend on what they desired to measure. Up to the present it was surprising to notice the diversity existing in the manner in which aberrations were expressed. He referred briefly to von Seidel's method. Aberrations might be expressed in four different ways. There were aberrations of focal length; of convergence; they could speak of lateral aberrations; and, finally, there were aberrations of deviation. This last, he thought, was the most important. According to the ordinary Gauss method of treating lenses, all light coming from an object lying in a plane was brought to a focus in the image plane. The light, therefore, gave

perfect focus, and if it departed from the simple path, indicated by the Gauss formulæ, then that departure was the aberration. In dealing with aberrations, they had two further matters to consider. Speaking generally, they might be considered objectively and subjectively, depending on whether the instrument was intended to form real or virtual images. Again, they required to be expressed both absolutely and relatively, in order to compare with other instruments of different focal length, etc. Finally, there was one suggestion about Professor Hartmann's method which he would like to make. Mr. Chalmers had alluded to the difficulty he had experienced in the matter of illumination, and had suggested that there was no harm in making the source of light rather large. Might he suggest to Mr. Chalmers that he might go even further, and adopt the method well known to process workers, *i.e.*, making the source of light so large as to produce an "optical V," by somewhat under-exposing his plate, and reducing and intensifying it, he would probably get a much finer dot with considerably less exposure.

Mr. Chalmers expressed his great indebtedness to all present for the kind expressions they had made concerning his paper. Some of the work he had referred to, though not original, was quite recent. One special point —the question of visual tests—was very interesting. Professor Hartmann himself always made visual tests for certain colours in examining telescopes, rather than attempting to take photographs on specially prepared plates. With regard to ready methods of examining and testing, Mr. Beck had put forward a suggestion which, though useful, did not exactly give what was wanted for spherical aberration. To get the exact focusing position it would be necessary to take two photographs, one behind the focus, and one in front of focus, with the same stop. Expressing aberrations by means of deviations did not give all one required.

OPTICS IN THE APPLICATION.

Some Workshop Problems and Processes.

Paper by F. Kendall, *a Member.*

Read at the Optical Society on March 17th, 1904.

WHEN I was invited by Dr. Drysdale to read a paper before the Optical Society, dealing with workshop problems and some of the difficulties of the practical side of the industry, I mentioned the matter to my employer, Mr. James Aitchison, with whom I gained my practical experience, and he readily gave me permission to do as I liked in the matter. It is only with reluctance that I have undertaken the task, having a full appreciation of the high standard that has been set in previous papers and a consciousness of my own limitations. Believing, however, that it is the duty of every member to promote the objects of the Society to the best of his capacity, and it being pointed out to me that it was not expected that one should be infallible, and that the expression of an erroneous opinion would, at the worst, only lead to criticism and the righting of a wrong idea, I have ventured the task of dealing with a difficult, and somewhat controversial subject, and I trust that as the opinions I hold for the moment are the result of my experiences, that they and the methods and conclusions advanced will be found of interest.

It is obvious that in the limited time at disposal, it is impossible to cover a very wide region of the subject, and in order to make my paper more generally useful, I shall, at the risk of being elementary, and, indeed, for several reasons, confine my attention to the manufacture of field glasses, more especially those of the prism type.

Firstly:—It is a portion of the industry in which, up to the present, we have not shone brilliantly against the continental manufacturer.

Secondly:—It comprises practically the whole art of the Optician, the perfection of work and the adjustment required in these instruments is the highest possible, and the difficulties and problems involved in their economical

manufacture are fairly illustrative of the difficulties involved in other optical instruments.

Thirdly:—It is the subject I am principally engaged upon at the present moment, and, therefore, ought to know most about.

The best way to place the subject before you in the shortest possible time will be to ask you to metaphorically descend to the level of the workshop, and, with some illustrations, go through the processes of manufacture,

FIG. 1.—THE GIMBAL GRINDER.

but before doing this I would like to clear the way with a few observations.

We are frequently told that the British Optical Industry is decadent, and that we are being outrun by our foreign rivals, and on looking round there is very much to give colour to the statement. We all have our different ideas as to the cause, and we all have our panaceas as a remedy. If we ask the mathematician the reason, he tells us it is owing to the entire lack of mathematical knowledge on the part of our practical workers, and if we ask the maker of automatic machinery, he tells us

it is because of our backwardness in the application of machine tools, while the technical education enthusiast will claim that by its aid industries are made prosperous, and that without it, sooner or later, these industries must be ruined.

These arguments are always built on a triangular basis. You first of all state that optical goods cannot be made good and cheap without technical education, and you reason that unless these goods can be made good and cheap we cannot trade successfully, therefore, the optical trade to be prosperous must have technical education. You can replace "technical education" in your argument by "cheap labour," "specialisation," "low rents," "a prohibitive tariff," "mathematical knowledge," "machine tools," and a host of other things, and you will find it sounds just as logical and true.

I have no fault to find with these deductions, but would like to say that too much can be made of these causes and remedies, if taken individually. The course of progress in the optical trade depends on many influences and agencies in which they are only factors. We have only to look at specimens of microscopic, telescopic, photographic and projection apparatus, gun-sights, and range-finders, turned out by English manufacturers, and it must be admitted that there is not a great deal that we need wear sack-cloth and ashes over. From whatever cause, however, the backwardness arises, we have got to take the situation as it is, and I shall endeavour to show that with all the disabilities we suffer from, the production of high-grade optical instruments on economical lines, although a difficult problem, is by no means an impossible one.

In beginning the production of any optical instrument in quantity, the first problem to be solved is, to find the smallest unit of output in a given time, that with moderately simple methods and careful workshop organisation, the most perfect standard of work can be obtained economically, and it is at this stage of our progress we see the great value of *simplicity of design;* the further problem arises as to how far *optical design* must be made subservient to commercial conditions. It must always be remembered that there is only one motive that should prompt the manufacturer to manufacture, and that is, the prospect of making a profit.

The scientist, the mathematician, and the inventor are nearly always men with great ideals, and the prospect of making some fractional improvement will always tempt them from the path of the least resistance, and in the aim at little perfections it is possible to so complicate the design of the instrument, that only by considerable specialisation and division of labour can it be produced at a price that will compare with similar instruments, perhaps as good, already on the market. The result may be that to produce them economically our output will be double the amount we can sell. It is obvious that this condition could not continue for long, and, therefore, we shall find that we have solved our problem of output the moment we settle the number we

FIG. 2.—CONTINENTAL POLISHING MACHINE.

can reasonably expect to sell (and sell them), and if fractional improvement can only be obtained by complication of design and uneconomical production, then the standard of efficiency has got to remain at that which best fits the commercial conditions.

It is at this point more than any other that a false antagonism arises between the theoretical and practical portion of the industry; it is not that either is wrong, but simply that there are commercial conditions that guide and influence one man with which the other has nothing to do.

The point to be particularly emphasised is the fact that the basis of all manufacturing, scientific, or otherwise, must be commercialism, and the value of anything

connected with it from design downwards, must be subject to its influence. It must be distinctly understood that this paper simply has reference to the production of the more popular optical instruments, such as field glasses, spectacles, etc.; it has no reference to the instruments of greater precision used for scientific investigation and laboratory equipment, where cost is not so much an object. In the latter case, I consider the English manufacturer, given a fair chance by those who control the purchase of these instruments, has no rival. And it

FIG. 3.—THE AITCHISON POLISHING MACHINE.

is principally in the manufacture of articles of the more popular character that the English Optical Industry is most backward.

On the first acquaintance with the simple methods of the optical glass worker, one experiences rather a feeling of contempt, and it is not until one has tilted at these methods several times with disadvantage to oneself, that one realises that, after all, the law of the survival of the fittest applies to workshop processes much the same as in other affairs. To see a man pushing a tool monoton-

ously on top of a post always fills anyone with mechanical notions with a burning desire to devise a mechine to reduce the work to a mechanical operation, yet there are conditions and classes of work that can be accomplished by this simple method economically.

The workman is always trying to work true spherical curves, and you wonder why he does not suspend his tools in some manner so that they could only move round the point of radius. The tools would always keep true, and the object would be attained without the expenditure of so much skill and energy. Many methods of apparently accomplishing this end will occur to anyone, and in our early stages we conceived the notion that if the tools were suspended by a gimbal they would always keep true to the curve required, and we devised the method as shown on the screen.

As an abstract notion this was excellent; the top concave tool could only move in the radius of the point of suspension of the gimbal, while the bottom tool could be brought up to it with any pressure desired while rotating, but, unfortunately, the idea did not come up to our expectations in practice, it was soon seen that the gimbal exercised no control over the tool, and a little reflection showed the absurdity of the arrangement.

The adhesion between the two tools, when polishing, is so great that it is impossible to separate them by a direct pull, and only a sideways sliding motion of the one tool on the other can be obtained. Now, if the gimbal is going to control the tools, it must be able to withstand the direct pull, and as it was impossible to devise unwearable bearings or points of suspension to stand the pressure, the gimbal points had to give way to the pull of the tools, and, therefore, exercised no control over their movements. The arrangement was useless, and soon went to increase the scrap heap. I, perhaps, ought to apologise for putting a failure like this before you, especially as I have one or two others to speak of, but in the workshop we frequently find that our progress is often accelerated more if we profit by our failures than if we achieve a partial success at the first attempt. It is from experiments like this that we draw our conclusions for further progress.

The abrasive and polishing processes in optical work are not, in any sense of the word, exact mechanical pro-

cesses, and cannot be reduced to mere mechanical operations, although the degree of accuracy dealt with, and the results obtained, are of a standard quite unknown in any other branch of engineering. It is impossible under any circumstances to devise a machine, however complicated, to produce a finished, optically true surface without the constant supervision of a more or less

FIG. 4.—SIDE VIEW OF ANOTHER AITCHISON POLISHING MACHINE.

intelligent workman, and any machines designed to carry out these processes have very little virtue unmixed with the brains controlling them. It is, therefore, necessary in solving the problem of economical optical production, to have a very clear idea in your mind of how much should be left to the man, and how much to the machine, and it would be easy to show that the

entire problem of economical production depends upon an exact appreciation of this question.

In the arrangement of the Aitchison Optical Works, the wish of our principal that the personal equation should, as far as possible, be uppermost, and the machine subsidiary to the man has been carried out, and we have been able nearly all through to reconcile a superior type of workman and simple mechanical pro-

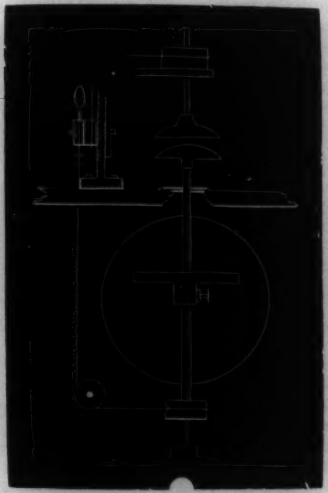

FIG. 5.—FRONT VIEW OF PRECEDING POLISHING MACHINE.

cesses with economical production; whether the results have justified the experiment others must judge, at all events we have been able to produce types of binoculars containing a great deal more work at a lower price than others on the market.

The importance of the problem of the relative position of the man and the machine in the workshop is so

much misunderstood that frequently we are told by those who are most enthusiastic about technical education, the advancement of the workman, and the enlargement of his mental horizon, that our only salvation is by making the workman a machine minder, or by supplanting him, as far as possible, by machine tools.

The object of any lens polishing machine is to produce a motion between the surfaces of the tools used, the more equal the motion on every part of the surface, the more satisfactory the machine will be. Theoretically, therefore, a machine giving a diametral stroke to both tools should be the best for the purpose, and dealing with flat surfaces and cylindrical curves this holds quite true, but the difficulty comes in when we require to get an equal motion between two segments of a sphere. The matter is usually compromised and a rotary motion given to the bottom tool and a diametral stroke to the top. Any machine that will give this motion, combined with a variable speed and a reversing gear, may be considered satisfactory. The polishing machine illustrated is a good example of this type, and is the type of machine in general use on the Continent; the chief advantages are that it leaves as little use for the intelligence of the workman as any machine for the purpose can do.

Bearing in mind the aim with regard to utilising, if possible, the intelligence of our work people, we were led to enquire whether this type of machine was the best for our purpose and whether we could not get an increased output by depending a little more on the man. After considerable experimenting we adopted a more simple machine in which we discard a diametral stroke altogether, and depend upon a rotary motion to both the top and bottom tools. This we find satisfactory for our purpose, and are able to turn out a greater quantity of work while occupying a smaller space than by the other method. Our machine is practically the same as that used for spectacle work with the exception that we rotate the bottom tool, and in spectacle work they do not. We are able, by judicious handling, to work a 9″ flat surface so that it does not vary 1/10 of a wavelength in any part of it.

In modern competitive manufacturing it is absolutely necessary that the retail price of the article should be

settled by the manufacturer, because all his calculations are based upon it. His output is regulated by the number sold, and the number sold by the price. The selling price governs the price of production, and the amount allowed for production influences the design of every part of the instrument, and every method adopted in its manufacture, and we see again how impossible it is to eliminate commercialism as our dominating factor. It is only upon a steady output that a workshop can be run economically, and it may be taken as a truism that spasmodic manufacturing never pays. The greatest amount allowable as cost of production will be half the selling price.

Bearing in mind these conditions, we can now start to produce our instrument, and, as an illustrative case, we will take a wide aperture prism binocular. Our optical system has been carefully arranged in this

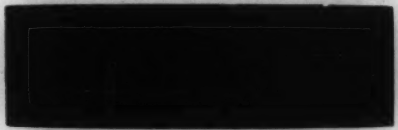

FIG. 6.—WIDE APERTURE SYSTEM.

instance by Dr. Drysdale, and it will suffice to say that it fulfils several conditions not previously attempted. It comprises an achromatic object glass, two total reflecting prisms, a field lens and an achromatic eye lens. The principal object in view in the design of the system is to make effective use of a wide aperture object glass, and obtain a much greater light value than is usual in this type of instrument, and also to increase the field of view in the higher powers.

The object glass is 40 mm. in diameter and 160 mm. focal length, the equivalent focal length of the eyepiece varies according to the magnification desired.

It will be seen from these measurements that the eight-diameter glass will give the best combination of power and light, for, by the law of minimum magnification we must put a power of eight upon our 40 mm. diameter object glass in order to reduce the emergent pencil at

the eye-piece to 5 mm., which is the diameter we calculate as the average size of the pupil of the eye in the dark. All the rays gathered by the object glass thus enter the eye. This rule is very important where it can be applied in the design of field glasses, but is sometimes overlooked. I have one example of a prism glass designed by a German maker with a large aperture in which the emergent pencil is greater than the 5 mm. The whole, therefore, of the objective is not effective except to a user with an abnormal pupil.

It may also be noted that we keep the focal length of the object glass the same all through the series; this gives us an advantage in the size of the field in the higher powers; it necessitates an increase in the power of the eye-piece as the power of the glass goes up, which

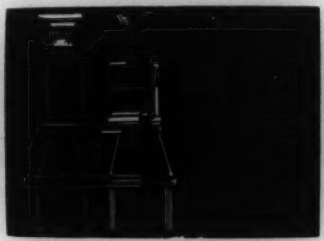

FIG. 7.—PLAN OF BODY OF WIDE APERTURE SYSTEM.

is against the rule in making prism glasses of the ordinary type. It is our custom in making these ordinary types to obtain the various powers by increasing the focal length of the object glasses, the eye-piece remaining the same all through the series; this is following the path of the least resistance which we like to stick to in the workshop.

The curves of the object glasses get greater as the focal length increases, and more lenses can be worked in a block in consequence. The cost of production within a certain limit is, therefore, inversely to the power of the glass. This is one of the anomalies of the industry. There are others quite as incongruous if you only knew them, and although one knows that a workshop experi-

ence for every Optician would be an advantage to the trade as a whole, it goes against the grain of a manufacturer to advocate it, because, if the Optician was more familiar with production, he would not be so willing to pay the great difference in price for the little difference in avoirdupois between the different powers of the ordinary pattern prism glasses.

Besides producing the optical system we must mount it in a frame that will give every facility for focusing

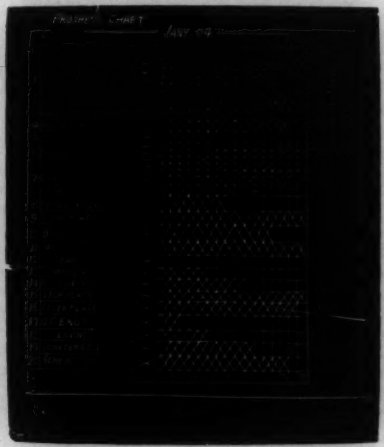

FIG. 8.—PROGRESS CHART.

and adjusting, and display to the best advantage the optical system. By careful design and experiment we have arranged our frame. It consists of 20 main components, and several subsidiary parts, and in order to facilitate our time and cost keeping we must arrange our component list, and number, name and value each component, and the amount allowable for production must be spread in proper proportion over the whole number.

The value given to each component can to some extent be fictitious, and it is wise to value them at something less than the price we have to produce them; our cost book being kept as a debit and credit account will show at once whether our methods are economical, for time and material will be charged on one side and deliveries from the workshop at the prices fixed on the other. Any component coming through at more than the price allowable simply proves that either our method of production or our design is at fault, and must be altered accordingly. It is also necessary that the rate of production of each component should be under control; to facilitate this we arrange a progress chart which is marked off at intervals by our foremen, and checked again by an assistant. We are thus able to see at a glance upon which parts we must concentrate attention when they lag behind.

Too much importance cannot be placed upon simple methods of exact cost and progress keeping, yet in the average workshop they are conspicuous by their absence, and unprofitable results often arise from neglect of this matter. On the other hand, it is possible in trying to check extravagance by too elaborate systems, to defeat the end by false economy; to keep the oncost charges, the clerical and unproductive staff of the workshop at the minimum point consistent with absolute efficiency is one of the nicest problems in workshop economics, which again is one of the most interesting branches of the much neglected and untaught science of commercialism.

We will arrange the production of the optical system first, and the only standard of work aimed at in this is perfection. Our tools can be accurately turned to the radius required by any of the methods in use, iron tools being used for roughing and brass for fine grinding. The next care is to produce the test plates for the curvatures. For each curve we produce a pair of plates, one with convex curvature, and the other with concave; these must fit so accurately that they exhibit only one broad colour band over the whole of the surface, when in contact.

The radius of curvature should be exact, but it will be found in practice that a variation of a few units in the third decimal of a millimeter in radius is not of

great importance, and the spherometer we use in checking our curvature is so sensitive that we can deal with these small quantities of curvature quite easily if desirable. With regard to the prisms, the angles must be exact and the surface must be absolutely optically flat. No curvature is allowable in the prism surfaces. The method of polishing that will enable us to obtain these results most easily will be "pitch polishing," in which the backing of our polisher that carries the minute particles of our polishing material (in this case, rouge) is

FIG. 9.—PRISM GRINDER.

pitch. It permits great control over the work and the use of a simple machine.

The prisms are slit approximately to size and shape on the slitting machine, and the sides are made exactly parallel. Many methods have been adopted with more or less success for getting exactitude with regard to the angles. Two discs rotating at right angles to each other enable two sides of the prisms to be ground at once. We adopted a very simple method of prism grinding by

fixing a guide at the end of an arm free to move across the plate, the sides of the guide being perfectly square with the plate, and the latter running perfectly true.

This method proved more satisfactory than the two discs at right angles. We were rather proud of the achievement and for some time ground our prisms in this way, but we soon found that a workman worthy the name became able to dispense with the guide and could do the work quicker without it. The equipment we now employ in prism grinding is simply skilful workmen, a rotating plate, and an accurate gauge. The fact is that in nearly all optical work there are many methods of achieving the same results, and the best way is to exhaust all the possibilities of one method before changing to another, and the cost book must be the only arbiter.

After the prisms have been trued, a number are blocked in an iron ring by the aid of plaster of paris and cement, and left to set quite hard and practically to become a solid block. These are further smoothed with very fine emery and placed upon machines for polishing; when polished they are knocked out, the angles gauged again for the second surface and blocked again, and the same process is gone through for the third surface.

With regard to our object glasses they are rough ground, trued, and fine ground, and put on the machines in the same way; there is practically very little art in the work until the final polishing stage is reached, and it is only by constant practice that one learns to know the point where a few more strokes of the polisher will either ruin the work you have already done or give you the most perfect results obtainable.

Some of the curvatures of our eye-lenses are so deep that only one lens at a time can be worked, and to get a satisfactory result, only the most careful work can be employed. It is at this point that much assistance could be given to the Optician by our scientific investigators, if they would turn their attention to the production of stable, high refractive index glasses. We could use much shallower curves, work more in a block, and produce at a lower price, although the glass might cost more. It is too much to expect of our only glass makers that they should carry out the necessary researches and

experiments at their own cost, but if a grant was obtained from Government through the influence of some of our scientific societies, perhaps the National Physical Laboratory could be persuaded to supervise a series of experiments.

Many optical improvements await the introduction of these glasses, and that they are possible is evidenced by the fact that we have glasses already with an index of 1.9. Although not commercially satisfactory, the existence of the diamond is further evidence of their possibility.

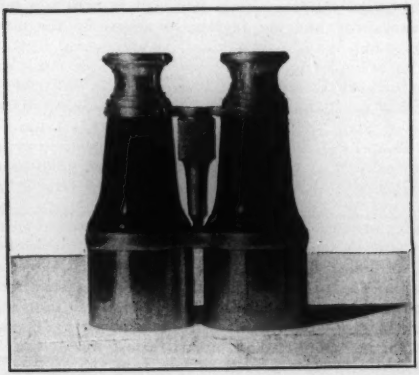

FIG. 10.—AITCHISON SOLID BODY FIELD GLASS.

The practice of optics is not pure science and cannot be considered apart from commercial conditions, and it is very desirable that there should be a much more intimate connection between science, as responsible for design, and the workshop, as responsible for production. A sufficient workshop experience should be included in the technical education of every student to enable him, whatever branch he may specialise in afterwards, to utilise and appreciate to the fullest extent the whole art of the Optician. It is almost impossible without a

workshop experience to realise the refined degrees to which glass working can be carried, if desired, and the effect of the absence of this experience is seen in many directions.

If the branch of the trade devoted to visual optics could only see this point, they would never be content with the standard of work that sometimes obtains, and they could score a distinct point over the oculist who cannot be expected, with his multitudinous other duties, to let himself down to a workshop experience. As long as the optical centre of a spectacle lens is set in the centre of the frame and the axis of a cylinder exact, nothing else matters; little attention is paid to errors of curvature, and the surface, as shown by the interference test, may be something like corrugated iron.

The examples on the test plate show how much this class of work is capable of improvement. It is useless blaming the manufacturer, for he will produce you any class of work you want if you only pay the price. If you compel the English manufacturer to produce at the same price that you purchase the surplus products of Continental and American workshops, he will do it for you, but you can only rely upon getting the same quality article. As far as I know the practice of working spectacle lenses to test plate does not obtain outside our own workshop, and with us not to the extent it should. The principal difficulty is the fact that most spectacle lenses are very thin and susceptible to flexure when blocked—but if blocked in a slightly different way to the usual method, a satisfactory result can be obtained with care, as will be seen from the examples.

Again, it is very rarely that either oculists or refractionists will avail themselves of the use of high refractive index glasses in lenses for visual purposes. This always seems strange to anyone constantly using them.

In telescope eye-lenses work, we frequently obtain a better result by using Baryta Crown Glass with an index of 1.6112, than we can obtain by using the more transparent Boro Silicate Crown glass with an index of 1.4944. We lose much more as regards definition by the increased curvature and thickness in the lens made with the latter glass than we lose by the absorption in the former. The reduction of curvature in lenses of 10 diopters and upwards is very perceptible, and a great

advantage is obtained in appearance. The complication of prescriptions with the index of the glass used would also tell in favour of the refractionist; on the other hand it would mean that the small Optician would become more dependent upon the large manufacturer.

The want of distinctiveness and originality in the design of the field glass the English Optician is content to sell, is also traceable to the absence of a workshop experience. It cannot in any way detract from the commercial aptitude of the Optician, if he is able to enter into the construction of the instruments he deals with, and it is difficult to understand why the same old patterns produced in the same old way should be for ever

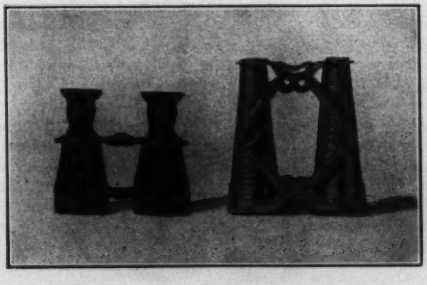

FIG. 11.—TWO EXTREME DESIGNS.

with us, except that they are designed to suit the methods of production of the continental manufacturers, and nobody else troubles. There is no reason why we should not impress a little character into the design of a field glass, the same as in other things. No one can ever mistake a Swiss watch for an American one, and why should not field glasses, sold by English Opticians, be distinctly English in character? I can make my point clearer, perhaps, with an illustration.

Here we have tried to design an ordinary pattern field glass possessing all the virtues of the foreign productions, in which we try to embody the solidity and distinct characteristics that should be obtained in all

things English. We make it impossible to twist the tubes out of the parallel by casting the body and bars in one piece. We dispense with the ordinary focusing motion, and the third bar, and accomplish the focusing by driving each object glass up and down its own tube, and as they are not connected in any way they cannot communicate any tilt to each other. The range and scope for originality in the design of field glass frames is unlimited. I give you two extremes, as an example.

The æsthetic sense is decidedly wounded by the over mechanical arrangement in the one, but the element of utility is so preponderant that it out-weighs any other disadvantage. The second is a jointed solid body—the

FIG. 12.—AITCHISON WIDE APERTURE PRISM BINOCULAR.

body and half bars being cast together—the eye-pieces focused separately. We use this form with very much higher powers than usually obtains in small glasses.

Judging from the little they teach otherwise, our scientific optical instructors seem to hold the view that the principles of design should be limited to the optical system. And once you have settled the arrangement of your lenses and prisms, the way you mount them matters almost nothing. It will surprise you to learn that in field glass making the optical system plays comparatively a minor part as far as the manufacturer is concerned. Generally, the cost of production of the body is from two or three times the cost of the lens system.

If the optical system has been carefully designed once for all, and methods of production settled, the end is practically reached, but with regard to the frames, it is always possible to make continuous variations and improvements, and, frequently, the success of a glass is contributed to, or detracted from, by the design of the frame.

To display the optical system to the best advantage and allow adjustments for width and focusing, necessitates the introduction of certain mechanical contrivances. Now, it is a very good principle to work upon in designing field glass bodies, to make the rule that all mechanical adjustments must, if possible, be out of sight. The average field glass purchaser has got his eye so trained to ordinary patterns that he is always suspicious about innovations, especially ofa mechanical character. Let us take the design of our wide aperture prism binocular.

It will be seen that the mechanical adjustments are very numerous. There is the focusing of each eye separately, the symmetrical movement of the prism boxes to get the different widths, the central focusing motion, and the motion for varying the aperture. It was no small problem to arrange all these motions so that they do not offend one's taste or become unnecessarily intrusive in the outward appearance of the glass, and it can be seen from some of the models upon the table how these motions are accomplished.

Discussion.

The President said he was sure all present would wish him to convey their thanks at the proper time to Mr. Kendall, but they themselves could probably better convey their appreciation of the paper by contributing the discussion on the various points raised in the paper.

Mr. Dixey said the first thing he had to say was to congratulate Mr. Kendall on his most interesting paper. He thought that some congratulation was at the same time due to Mr. Kendall's firm for the work they had been engaged in during the past few years. They had certainly succeeded in putting upon the market several original and most interesting types of field glasses. He referred not only to the new prism glass, but to several other original models, such as the little glass with a

jointed centre and adjustable heads, which they had just seen on the screen. Nothing involved in the glass was absolutely new, but the whole thing was well put together and looked serviceable.

Mr. Kendall had referred to the 5 mm. limit for the diameter of the pupil. This was a very important point, but one which was often only carelessly looked at by designers of instruments. And not only by the designers but also by the users of the instruments. He agreed with the lecturer that 5 mm. was large, and that for working purposes the limit might well be taken as less. But under some conditions, it might be taken as larger. He suggested, for instance, that for night naval work, such as they had been reading about lately, it might be an advantage to increase artificially the diameter or aperture of the pupil, and then, by the aid of a good light-collecting object glass, and a wide emergent pencil, to make the most of what light there was under those particular conditions.

In regard to spectacle lenses, he thought people were sometimes inclined to be too "finnicking." There was a limit of permissible error in everything and with regard to spectacles, the numerical value and probable effects should always be estimated in considering an error. Referring to the polishing of lenses, Mr. Dixey would be glad if Mr. Kendall could suggest some practical and simple workshop method of testing the polish of a spectacle lens. His (Mr. Dixey's) present method was to examine with the naked eye the reflection from the surface. He thought there must be some more satisfactory method. It must be a quick method, however, not involving any elaborate apparatus. The density of spectacle lenses, referred to by Mr. Kendall, also constituted a subject of great interest, though he did not regard its bearing on the question of spherical aberration as of much importance, the error, even in deep lenses, due to that defect, being inconsiderable.

Mr. Chalmers, referring to the suggestions put forward by Mr. Kendall with regard to the varieties of glass used, said he was afraid the flint glasses would not be sufficiently hard to be used for visual purposes, nor the surface so permanent. The price might also be a consideration. He was glad to see that the lecturer had called special notice to the necessity for attention

being paid to the mechanical side of the design of optical instruments. In all optical education an attempt should be made to include in the training of the Optician some more purely mechanical knowledge. He was hoping that in course of time there might be something more done in this direction at the Northampton Institute. If there were any special directions in which any members of the Society would like to suggest developments in that direction, he would be very pleased to receive such suggestions. At the same time, he would be very pleased to receive any suggestions from members of the Society for the expansion of the classes, and especially in the direction to which Mr. Kendall had referred, the increase of mechanical knowledge.

Mr. Aitchison felt rather diffident about introducing any personal remarks in regard to the paper they had listened to. He spoke very warmly in praise of Mr. Kendall, who, he said, had borne the brunt of the work connected with the Aitchison optical works. His original idea of starting a workshop arose from the fact that he was beginning to get disgusted with German competition. It seemed to him that the whole English market, and particularly in field glasses, was getting into the hands of purely German agents with German goods, German names and German prices.

He therefore took the matter up and decided to try and do something. When he started he did not anticipate the difficulties they would meet with, but with the aid of the best mathematical knowledge they had been successful. Looking at the matter from a business point of view, he might mention that it was only after three years' labour that he was enabled to put the glass on the market. The Germans had been watching him, and within three months of his putting the glass before the public, they met the anticipated competition by reforming their prices. He did not want to introduce the fiscal question into the matter, but that was an illustration. So long as we did not manufacture things in this country, the foreigner could fix his own price, and the public had to pay. The moment we attempted to start a factory, he (the foreigner) made a reduction. In regard to what Mr. Dixey said about errors of aberration in spectacle lenses, he did not think that question had ever been raised by oculists. So far as his experi-

ence went, he had never heard of an oculist questioning the accuracy of spectacle lenses so long as the dioptric power was approximately correct. The ophthalmic-surgeon was intensely critical of the Optician when he could show that the latter had made an apparent error of even a quarter of a diopter in prescribing for spectacles, but he (Mr. Aitchison) had seldom heard of one who took the trouble to investigate whether his prescriptions were carried out correctly in regard to material of glass used, polish of surfaces, etc.

Mr. Ryland briefly touched upon the question as to how far the working man should be educated. There was also the question as to the direction in which the practical optical worker should be trained. As a rule he thought he should be more of a mechanic and an engineer than the Optician proper, for it was very seldom that he got the opportunity of looking through his glasses.

The President, in inviting Mr. Kendall to reply, said he should like, in the first place, to express his appreciation of the paper and of the energy shown by his firm in putting on the market such instruments and in attaining, as he thought they had, so very great a degree of success. He could only hope that the commercial results would be as satisfactory and as profitable. He did not think it was necessary to discuss at any length the earlier part of Mr. Kendall's paper. They need not attempt to decide whether the mathematician was altogether to blame or the practical man. What they had to do was to endeavour to combine the work of the Optician and that of the practical man. That was the secret of success as evidenced by their German compeers and rivals.

He had recently been looking up the returns of the Board of Trade for optical instruments, and while it appeared that the export from British sources had distinctly increased during the past few years, the imports had increased in a greater ratio. Therefore, he thought it was of very real importance that methods such as those described to them should be put into practical use, and be generally recognised in English workshops. He was glad that Mr. Kendall had thrown on the screen one of his failures, because he believed greatly in the educational values of failures. He agreed with what

Mr. Chalmers had said about glass, but he thought that one of the main objects of research was that suggested by Mr. Kendall in his paper, namely, to endeavour to discover a glass having the properties of higher refractive flint glass, so far as refractivity was concerned, yet possessing the more suitable properties and services possessed by some crown glasses. Could that be accomplished, Mr. Kendall's hopes would be realised, and Mr. Chalmers' fears done away with. The remarks relating to the relation of the worker to the Optician appeared to him as being extremely valuable and important. He should like to ask Mr. Kendall whether he could give them some practical suggestions whereby that same intimate connection they all desired could be secured. That was a point to which he thought the attention of the Optical Society might particularly be turned, and he (the President) should be glad if, during his year of office, he could help in any way to securing the very admirable aims put forward by Mr. Kendall.

Mr. Kendall, in replying, said he believed this was the first time that the Optical Society had given anyone who could claim an acquaintance with the workshop an opportunity of expounding his views, and he was afraid he had scarcely done justice to the occasion. He did not expect the matter of his paper would be accepted without criticism, and he was aware that other experiences, by other people under other circumstances, must reach other conclusions. He did not, however, see how the general advancement of an important industry like their own could be brought about unless the various elements composing it got the opportunity of exchanging experiences through discussions like that.

In considering the optical industry as a whole, he looked upon it as composed of three elements, more or less indefinite and opposing. These were quite clear in his own mind, and he labelled them: Science, Commercialism, and the Workshop. In his view the prosperity of the industry depended on how far these elements could be made to agree. He did not think anyone would dispute the fact that any particular industry could only prosper just as far as it was profitable, and, before anything else, a workshop must be run on a sound commercial basis.

It always seemed to him to be just as necessary that

their scientific teachers should have some knowledge of the Art of the Workshop and the Science of Commercialism, as it was for the workman himself to know something about the physical facts and principles underlying his occupation. He did not see how their scientists could exercise the guiding influence on their industry that they all desired, unless they allowed a place in the equation for these two other factors.

Optical progress would be much more advanced by their scientists letting themselves down to the workshop than by trying to elevate the rank and file of the workshop to the position of the scientist. Some portion of the workshop had got to be content in the sphere of life it had pleased Providence to call them, and his candid opinion, after some little experience with the workshop, was that higher education of the masses ought only to be advocated with a good dose of the catechism. Workshop systems were nearly always sufficiently elastic for the best man to find his way to the top.

He attempted in his paper to show that optical work was not so very difficult after all, and he thought it a great mistake to cultivate the idea that the absence of great theoretical knowledge prevented the attainment of satisfactory results in the workshop. At all events, if such masters of the Art of the Workshop as Alvan Clark, Mr. Brashear and Hartnack, of Potsdam, were classed as trial and error method men, then the ordinary man could congratulate himself that he sinned in good society.

For his own part he frequently had a difficulty in appreciating the fine dividing line where empirical methods ended and scientific ones began, and it was always difficult to convince an adept at the art of figuring of the need of very refined degrees of calculation for the elimination of spherical error. No less an authority than Sir Howard Grubb, speaking of this subject in a lecture at the Royal Institution, said that he would undertake to alter the curves of the crown and flint of any of his objectives, by a large quantity, increasing one and decreasing the other, so as to still satisfy the conditions of achromatism, but introducing theoretically a large amount of positive or negative spherical aberration, and yet make out of the altered

lenses an objective perfectly corrected for spherical error.

It was frequently possible to make a better objective by deviating from the curve, which gives a true correction for spherical aberration, and correcting that error by figuring rather than by strictly adhering to the mathematically true curve. A mechanical mind prefers a mechanical method, and a scientific mind a scientific one; each had its own sphere of usefulness, but undoubtedly the ideal condition was when the two were combined together.

Mr. Dixey's suggestion with regard to the increase in the diameter of the pupil by a mydriatic would be very useful in some instances, but he did not agree with him that there should be no limit of error with spectacle lenses. As a matter of fact, so far as the retail Optician was concerned, the profits on spectacles were widely different compared with field glasses, and the amounts they had to play with in producing field glasses were very small compared with the amounts in spectacle work. In achromatic object glass making they had to work 12 surfaces for 5s. If, then, they could work 12 optically true surfaces for an opera glass for 5s., why not twelve spectacle lens surfaces for the same amount?

With regard to spherical aberration in spectacle lenses, he had found that a more important matter than Mr. Dixey seemed to think it was. He quite agreed with Mr. Chalmers as to the liability of the higher refractive index glass to scratch. If spectacle people would only go in for the use of this glass, they who used it for other purposes would be able to get it very much cheaper.

THE WORK OF THE OPTICAL SOCIETY.

CO-OPERATION, EDUCATION, STANDARDISATION.

PRESIDENTIAL ADDRESS BY DR. R. T. GLAZEBROOK, D.Sc., M.A., F.R.S.

Delivered before the Optical Society on April 21st, 1904.

THE fact that changes of a far-reaching character are necessary if England is to retain its position in the van of industrial progress is one which is being brought home to us in many ways, among others by the recently published report of the Moseley Commission to America.

"Looking into the future of our own country," says Mr. Moseley in his interesting preface to the report, "I feel bound to record my belief that the régime of the past, however successful it may have been, is obsolete. Honesty, doggedness, pluck, and many other good qualities possessed by Britons, though valuable in themselves, are useless to-day unless accompanied by practical, up-to-date scientific knowledge, and such knowledge only becomes possible with an enlarged and enlightened system of education such as the United States possesses. I am personally convinced that if the British Empire is to hold its own, our old industrial methods will have to be dropped, and especially our present method of popular education will have to be changed."

To-night we are concerned with the optical industry, but Mr. Moseley's general statement applies to it. There can be no doubt, I fear, that in recent years its rate of progress in foreign countries has been greater than in our own; thus, while in the past five years the annual value of our exports to foreign countries of instruments and apparatus, surgical, anatomical and scientific, has risen from £228,000 to £280,000, the value of our imports has grown in four years from £710,000 to £759,000, while in 1902, owing to an abnormally large importation from America, it reached the enormous total of £948,000.*

The fact of German success is continually being paraded before us. The cause does not lie in the greater

* If we go back to 1897, the difference is more marked still; the exports rose from £160,000 to £280,000, the imports from £99,000 to £759,000; but no separate records of imports were kept before 1900, and, no doubt, some portion of this apparent rise is due to this fact.

skill or ingenuity of the German workman—British honesty, doggedness, and pluck are still effective assets on our side—but in the fact that elsewhere the value of science as a commercial factor is more fully realised than with us. Let us see how they have realised it in Germany and in France, and then ask ourselves what can we do to profit by their experience.

I will return later to the efforts the Optical Society has made to apply science to its industry, with a view to considering how in my year of office as your President we may strengthen those efforts and make them more fruitful.

The special catalogue of the Joint Exhibition of German Mechanicians and Opticians at Paris in 1900 has often been quoted to you. Let me again, however, at the risk of wearying you, read a few lines.

"At the commencement of the 19th century the French and English makers of scientific instruments were far in advance of the German." . . . . . In Germany it is only within the last twenty or twenty-five years that the State has espoused the interests of the home industry in scientific instruments, but such have been the efforts and the results that the position has, at a blow as it were, changed in favour of Germany. . . . .

"Soon, also, the necessity was recognised of the close co-operation of the scientific and practical men. Accordingly, in 1879, several scientists, mechanicians, and Opticians united in Berlin and formed the nucleus of the German Association of Mechanicians and Opticians, which was formed in 1881, and embraced the whole German Empire, having for its object the scientific, technical, and commercial development of philosophical instrument making. The official organ of this Society, the 'Zeitschrift für Instrumentenkunde,' was likewise founded in 1881, and is devoted to the theoretical and practical development of scientific instruments.

"Specialised schools were established, first in Berlin, then in Frankfort-on-the-Main, and subsequently in many other towns where savants and practical men are combined in training the rising generation in the theoretical departments of the subject. As a result of these serious scientific aims, German mechanicians and Opticians sought in their laboratories and workshops the assistance of scientists, and at the present time the

majority of the leading German firms retain one or more experienced mathematicians or physicists in their permanent service.

"The greatest share of the impetus given to the manufacture of scientific instruments, however, is due to the Imperial Physical and Technical Institute, which was established in 1887. . . . . This Institution has already done great service, and a large proportion of recent progress is due to its stimulating and helpful influences."

This very frank statement—we need not admit all it claims—brings into prominence three factors of success: co-operation, education, standardisation.

The success of the German exhibit at Paris—and it was a splendid show—aroused our French confrères, and in 1901-1902 the Committee of the Association of Manufacturers of Instruments of Precision published a joint catalogue, with an introduction by the late Professor Cornu, who, after pointing out the danger which had threatened the French industry and praising the skill with which it had been met, concludes:—

"To complete your successful dispositions for the struggle, it remains only to adopt the powerful weapon of association and of self-control—that self-control willingly adopted in view of the general interests; an intelligent union directs all efforts to a common end, instead of employing them in those barren struggles which eagerness after immediate interests arouses in short-sighted minds."

Professor Cornu realises to the full the truth, old as the days of Æsop, that joint efforts are necessary to secure a common end. I am glad that at any rate in some sections the British exhibit at St. Louis this year is to be a joint one; and though our progress in England has been slow of recent years, we remember with interest, perhaps also with some sadness, that it was after visiting our loan collection of scientific apparatus in 1876 that Professor Abbe realised that the right way to attack the problem of the microscope was through the glass works.

Co-operation, education, standardisation. What has the Optical Society done to promote these factors of success? The work of the Society is carried out largely by co-operation; it consists now of some 220 members,

practically all closely connected with optical work; the papers I have listened to during the short time I have been connected with the Society have given me a high idea of its aims; the discussions have shown the very real interest which animates its members.

Already I think there is a wiser feeling recognised in the trade. Its members are less afraid of each other; friendly intercourse has rubbed off some corners, smoothed away some difficulties, and taught all the lesson that each has much to learn from his fellows. Competition is not only with your neighbour across the street; that with friends from over the sea is more serious, and can be met best by co-operation.

Local societies have sprung up, connected in a more or less formal manner with our own; the industry is stronger than it was when the first report appeared in 1899.

And now we aim at co-operation on a larger scale. About a year ago a Committee was appointed to ascertain and report to the Council what other bodies will act with the Optical Society in the matter of organising a Convention of Opticians, and what members of the optical industry and others interested in optics would support the movement.

The Committee has held several meetings, and in response to a circular issued to a number of representative bodies and firms has received sufficient support to make it desirable to proceed further in the matter. Accordingly, a meeting has been summoned for next Tuesday to arrange for some definite action, and, if thought desirable, to appoint a committee to organise the Convention and determine the questions that should be brought before it. It is thought right that this committee should be entirely independent of any existing organisation.

Let me indicate very briefly some of the matters which might come before such a Convention. The question of standardisation I will refer to later, but it is clear that it would be desirable to arrange, if possible, that decisions which we may arrive at should be discussed by a larger public, and should receive a wider acceptance than is possible if they are confined to our Society only.

The steps desirable to organise the technical educa-

tion of the Optician need very careful consideration. Useful opinions might be elicited by a judicious paper, and discussion at the conference. The question of an English exhibition of optical manufacturers and processes also deserves consideration, but let me say that I trust that if any such exhibition is organised it may be arranged to show the joint result of English industry and skill, and not merely to expose the wares of rival houses.

Let me give an illustration of my own experience in 1900 at Paris. A generous supporter of my laboratory had placed a sum of £1,000 at my disposal for the purchase of apparatus, and I was instructed to go to Paris and investigate how to spend it. I went first to the English section. I saw a number of glass cases arranged like shop-windows, with beautiful apparatus behind the glass, and I was met by a stalwart representative of the London constable, Division A, some 6 ft. 2 ins. in height. I see a dozen or twenty similar splendid men have been sent to St. Louis for a like purpose—who guarded everything with splendid fidelity and jealous care. To unlock a case or let me handle a piece of apparatus was a thing unheard of. Robert was very ready to gossip of Paris, and the dulness of his lot shut up in the Exhibition building all the day, but as to helping the exhibitor to sell his wares that never entered his head.

I passed on to the Swiss Exhibit. There was a notice to tell me that M. X. in the Bureau would give information as to the goods of which I was in search. On enquiry, M. X. was ready to open the cases, to let me handle the instruments, to explain their action. One instrument of which I was in search was not in the Exhibition. A note was taken, and in the course of a week or so full information came from Zurich.

The same happened in the German Exhibit. The catalogue was handed me, and I was told to mark the instruments I wished to examine, and opportunity should be given me of seeing all. One day a representative of Messrs. Zeiss met me to discuss optical instruments, on another day I had the metrological and electrical apparatus at my disposal. It is sufficient to say that in the end a large fraction of my £1,000 was spent outside England.

I think that a representative exhibition of English

instruments, arranged to show the best that we can do, would be of distinct service to the industry. Perhaps some such may come out of our proposed Convention.

Education.—The Optical Society has realised the importance of this, and in co-operation with the Northampton Institute has already taken steps of real moment. The need for education was put most clearly by Professor Thompson in his address at the Society of Arts, just two years ago, and the discussion showed how fully the need was realised. Your Council in 1901 proved, by the appointment of a special Educational Committee, that it appreciated your responsibility in the matter, and the case for a large effort was put before the Technical Education Board in a statement read by Mr. Conrad Beck, which presents the matter in the most admirable manner.

Meanwhile, not very much has been done, the change in the Education Authority for London has caused delay, but it is understood that the Technical Education Board has passed on the proposals, with a strong recommendation for the favourable consideration of the new authority.

Some progress, however, has been made; there is now a special Optical Department at the Northampton Institute, and valuable experience is being gained which will be of real service in preparing for a larger scheme.

The report which Mr. Aitchison, to whom the cordial thanks of the Society are due for the energy and self-sacrifice he has displayed in promoting the scheme, laid before our meeting in February, is most hopeful reading. During the current session the number of students is 135, and the class entries 454.

He was able to say that "the assistant class seem to bring a greater degree of intelligence to bear upon their work, and they now see that, other things being equal, the man who has the greatest amount of knowledge of scientific optics is the man the employers want, and are determined to have. I must confess that I have been both surprised and pleased to see the zeal with which men who have family ties and associations to distract their attention after working hours, still find time to put in a fair amount of study in the classes at the Institute."

The cost of these classes has been about £1,000, and towards this the trade has contributed a sum of one

hundred guineas. But good work though this is, Dr. Walmsley and Mr. Aitchison would be the first to admit it needs great extension. Dr. Walmsley, we know, has large ideas, and a recent journey has not tended to limit his horizon.

At a fitting time the new Educational Authority for London must be reminded of the urgent need for a larger and more complete organisation, and it will be for your Council to consider what part the Optical Society must take in forwarding the good work they have started.

New buildings will be erected, while a larger staff and apparatus and machinery of a modern kind must be supplied. Education, of course, is not everything, but it is essential; let me remind you of President Roosevelt's remark to the Moseley Commission: "Education will not save a nation, but no nation can be saved without education."

Therefore, let the Optical Society insist that the necessary education be provided, and to show their zeal let its members continue to help as heretofore in that provision.

We turn now to the last of our three factors of progress: "Standardisation," the application of scientific methods and standards to the organisation, and the checking of your work. Here, also, the Society has not been remiss. The various sub-committees of the Optical Standards Committee have had many meetings. The interim report presented to-day, which is to be discussed next meeting, was not arrived at without much solid work. There are other questions still under discussion, which will be brought before you in due course, and I venture to think that when the complete report is issued a very notable and important result will have been achieved.

I need not discuss with you the importance or the necessity of standardisation; they are obvious, a visit to two shops, in one of which the modern practice of repetition work is adopted while in the other ancient methods are adhered to, is sufficient to convince the doubter.

But to advocate repetition work, to issue reports, to frame definition, is not enough; you need material standards in many cases, and in all you want the means of testing your results and of discriminating between,

shall I say good, better, best, or good, bad, and indifferent?

In Germany, as we have already been told, "a large proportion of recent progress is due to the stimulating and helpful influence" of the Reichsanstalt. I desire that a future President of the Optical Society may say some years hence, when reviewing the progress of your trade, that a large proportion of the advance is due to the work of the National Physical Laboratory.

At present we have not done much, our optical equipment hardly exists. If you pay us a visit, as I hope you will, this summer, it will be to spy out the nakedness of the land, but the fault, as I have already told you, is in part your own. The demand for improved optical tests has hardly been sufficient to justify a large expenditure on the part of the Committee.

I welcome the request made to me some little time since to organise tests for Opticians' test cases, and I trust the Committee, which has been appointed to discuss the details of the tests, may be the forerunner of others; but to carry out such tests I must have your support in the appeal which must be made to the authorities for funds, and I must have your practical help in devising and in making the instruments and apparatus required.

For the latter, I have to thank some of you already. Let me give you two instances: A well known Optician was visiting my laboratory some few days since, and, after expressing his interest in what he had seen, said he must have his grumble. My photographic lens testing apparatus was behind the time. I put a bold face on the matter, admitted the fact; said I had no money to buy a new one, which would cost £100, and asked if he would give it. I will give you half the cost, was his generous reply.

Another firm wrote recently from Manchester, offering a liberal contribution to the cost of apparatus required for optical tests. When it is once realised that your trade requires standardisation, it must come, and I shall esteem it a privilege to work with you to secure it.

It seems, then, that we are beginning to appreciate the importance of those three factors, Co-operation, Education, Standardisation, which have already proved so beneficial abroad. The Society is working on the

right lines; it will be the business of the officers and Council during the session now in progress to see that it goes forward and prospers.

Mr. Hyatt-Woolf said he rose, not, of course, for the purpose of criticising the address—that would be extremely unorthodox, and were he moved to do so there was very little room, anyhow, for criticism of an adverse nature. He complimented the Society on having such a President as Dr. Glazebrook, and stated that the point which particularly pleased him about the address was the reference to an Optical Convention, of which they would hear more at the preliminary meeting to be held on the following Tuesday, and which, he hoped, they would all attend. He reminded his hearers that a Convention would not be, as usually assumed, the first of its kind in this country, although, probably, the previous Convention was not regarded as such by those who convened it. Still, it had a very great effect upon the industry. The meeting was held some 14 years ago, and the outcome of it was what ultimately came to be known as the "Battle of the Bridge."

No doubt all his hearers remembered this famous astigmatic bridge case in which one particular firm claimed the monopoly of what had since become a very important department of the industry. In that case a large number of optical houses were represented, and a scheme, not dissimilar to that suggested by Dr. Glazebrook, was then discussed. Nothing very tangible, however, came of the meeting, but still the seed was sown, and it was out of it that had sprung up, in a way, the present work. Another of the main features was the outcome of trade journalism, and from that had come all their present progress, although trade journalism itself had not met, in certain quarters, with the support it merited. Dr. Glazebrook had reminded them that if they did not go ahead they must go backwards. The position they occupied was comparable to a train standing on the railway lines. If they did not go forward they would either be shunted on to a side track or knocked off the line altogether. What they now wanted was a leader, somebody of energy and enterprise, of determination and originality, to organise their movements for them and to lead them with words and actions, and such a man had been found, he thought, in their

President. He looked forward to very great advances in the industry. Mr. Woolf, in concluding, proposed a vote of sincere thanks to Dr. Glazebrook for his address.

Mr. Aitchison, in seconding the vote of thanks, said he was sure they all felt very much the honour Dr. Glazebrook had conferred upon them by accepting the Presidency of the Optical Society for the present year. The influence which such a President would have on the work of the Society, and through the Society on the trade, would be very great indeed. One of the great drawbacks in England had been, he thought, the lack of sympathy and intercourse between the leading scientific men in the kingdom and the actual workers, and he trusted that the advent of Dr. Glazebrook would form a precedent for other leading scientific men to join the Society. This would lead to that state of progress which was so evident on the Continent. With regard to the contributions that the trade had given towards the funds of the Northampton Institute, it might be understood from Dr. Glazebrook's address that only 100 guineas had been subscribed. As a matter of fact, 300 guineas had been subscribed during the past four years.

Dr. Walmsley heartily agreed with all that had been said, and desired to personally express his own thanks to Dr. Glazebrook for his address. To his mind, the most interesting part of the paper was the reference to the lines on which the Society had been conducted since its start, and remarked that the organisation was due to a desire for co-operation in the minds of a few members. He most heartily thanked the Society for the assistance given to the Northampton Institute, and not only for the monetary assistance but for the advice and hints tendered by different members of the Society as to the conduct of the work at the optical classes carried on at the Institute.

Mr. Hyatt-Woolf then put the following motion:—That a very hearty vote of thanks be passed to Dr. Glazebrook for his address. The motion was unanimously passed.

Dr. Glazebrook, in reply, said he desired to thank them all for the way in which they had received his address. There were a few other matters he should, perhaps, like to refer to briefly, and a little less

formally. In the first place, with regard to Dr. Walmsley's remarks as to the lines on which the Society had been founded and on which it had been worked. On looking through the reports of the Society and the papers which had been read at its meetings, he had been greatly impressed by the wisdom of those who controlled the Society, and the useful way in which they had guided its work. It was after reading through those reports that he selected the title of his address. Then Mr. Aitchison spoke very kindly about what he had termed the honour done to the Society by his acceptance of the Presidency. He assured them that in so doing they were only enabling him to realise in part what had been his ideal for many years back. During the period when he was at Cambridge, both as student and teacher, he had a feeling, as soon as he realised the position of English industry to English science, that the two should be brought into closer touch, and when that opportunity arrived, in the establishment of the National Physical Laboratory, of bringing scientific knowledge and skill to the aid of the industrial workers, he felt that he was taking one step towards the work he had long had in mind. He realised most fully that the work of the National Physical Laboratory would not be of real value unless those who controlled it, and who carried on the work, were thoroughly trusted by the industrial side of the nation. He accepted it as a pledge of this co-operation between scientist and worker, that they looked to the National Physical Laboratory to bring, in time, some real assistance to the industry, and that they had also elected him to the post of President. He also asked them for their co-operation as practical Opticians, and was sure he would obtain it. He also asked for co-operation in another matter. He had been grieved to find that the numbers of the Society were not quite as large as they were a year ago. He was sure that there were many men in London and in the country who would profit by becoming members of the Society, and he thought all present would be doing good and useful service if they could say that they had brought a friend to join the Society. That would be a good example of co-operation.

BINOCULAR VISION AND THE THEORY OF THE STEREOSCOPE.

PAPER BY T. W. BARBER, M.I.C.E.

Read before the Optical Society on June 21st, 1904.

HISTORICALLY, the general facts of the binocular vision have been known for hundreds of years, but have only been scientifically investigated about the years 1839-1846, by Wheatstone and Brewster, between whom a good deal of controversy and difference of opinions existed. Wheatstone, who described and introduced several forms

FIG. 1. FIG. 2. FIG. 3.

of stereoscope, held the opinion that, although, owing to the distance between the eyes, each eye projects upon the retina an image differing materially from that of the other eye, yet that these discordant images are blended into one concordant one by some elastic movement of the retinas or by means of the nervous apparatus. Thus, he affirmed that if two straight lines were drawn on paper at a distance of two or three inches apart, these lines could be blended into a single one by the eyes or by a stereoscope even if the lines differed considerably in length or were inclined at different angles within certain limits.

Brewster denied the accuracy of both the fact and the explanation, and there is no doubt he was justified in so doing; but he also fell into error in his explanation of the phenomena and principles of binocular vision, as I shall show presently, by affirming that the solid perception of binocular vision is due principally to the convergence of the axes of vision.

In investigating this subject we shall have first to consider the optics of the human eyes and their capabilities. It occurs to us at the outset that our eyes are differently placed to those of the majority of the animal creation, and that from these differences of position, as well as from differences of structure, there must be great variations in the effective vision of various classes of animals. Without going into the physiology of the question, it

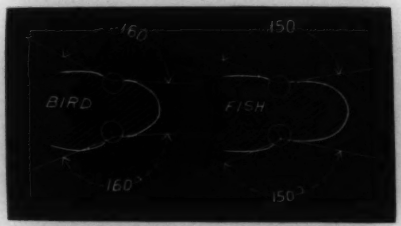

Fig. 4. Fig. 5.

may be said that our eyes are in duplicate, and their nervous apparatus is so connected that the vision of one eye exactly (in sound, normal eyesight) supplements that of the other.

Two pictures are produced on the retinas, and by the optic nerves are apparently blended into one, and this results from the position of the eyes, both of which are in front of the head, and point in nearly the same direction, but not quite, as we see by the diagram No. 1, which is a horizontal section of the eyes and nose, showing the centres of visual angles and the angles included in the field of each eye horizontally.

From this diagram we observe that each eye takes in an angle of about 120 degrees (this varies in different subjects), and that the right eye includes about 35

degrees more to the right, and the left eye to the left than the other. These portions of the total field of view, therefore, are monocular, and appear the same to us whether one or both eyes are open; binocular vision on these margins being cut off by the bridge of the nose.

In this connection it is interesting and important to note the variation in this overlapping field of view that occurs in animals other than man, by comparing similar horizontal sections of their heads as given in figs. 2 to 5. These figures are very instructive in this way. They show that man is the only animal who with both eyes takes in nearly the same field of view, and who, therefore, possesses binocular vision; that is to say, who sees the same view duplicated, and, therefore, about twice as brilliantly, as must be the case with the lower animals, unless there is something in their unduplicated vision which gives them superior brilliancy of the single image to compensate for their lack of duplicate vision.

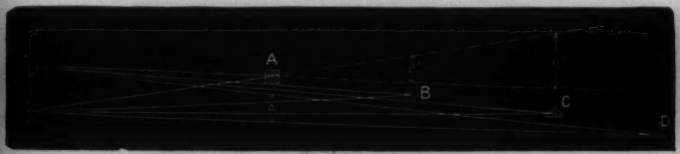

FIG. 6.

Brewster, however, asserts that the vision of our two eyes is only 1/13th brighter than that of one eye alone, on the authority of Dr. Jurin; but in the author's practice and experiments he has proved that the binocular picture is nearly double as brilliant as the monocular one, and is supported in this view by the great superiority of binocular telescopes, opera glasses, and microscopes, which, if they gave only 1/13th greater visual intensity than monocular ones, would possess very little superiority.

Brewster's assertion that the irises, when both eyes are used, contract to admit less light than when one eye is used, is entirely unfounded. The irises adjust themselves automatically to the amount of light present, and not to the amount to be averaged between the two eyes. This may easily be tested by trial with a looking glass,

opening and shutting one eye, when it can be seen that the iris of the open eye does not alter in size.

That the lower animals have compensation for their monocular vision is beyond doubt; for their sight in many cases is superior to ours. In some cases its superiority is confined to comparatively near objects, in others to great distances. These specialities are accounted for by peculiarities of their optical formation, enabling them to focus more sharply, and with what a photographer would call a larger aperture on such near or distant objects. Habit and mode of life determine the specially concentrated power of their vision. The irises of their eyes, from their great size, indicate such abnormal powers of vision. The eagle, for instance, is said to be able to look at the glowing sun without blinking, and can also see very minute objects at distances for which we should require a telescope.

Insects, again, have eyes which, having facets like a diamond, give a vast multiplicity of images, each image, however, from its angle, differing slightly in field of view from other images, yet there are good reasons for believing that these multitudes of images are blended on the optic nerve into a single one; the minuteness of each image is probably in this way reinforced by the numerous apertures into one brilliant view, as a compensation for the smallness of their optical systems, and the small amount of light that would be admitted to each.

It must be noted here that the centre line of each field of view of the eyes of animal or insect is not necessarily the centre of the angle of vision, for all animals, including men, have the power of concentrating their vision on any point within the field of view. In birds and fishes, however, as their fields of view do not overlap (see figs. 4 and 5), it is evident that even with this power of concentration within the field of view no binocular vision is possible to them. This brings us to the next point, this concentration of vision; a faculty totally unaccountable on the principles of ordinary optical instruments.

The eye may be considered as a camera, which it strictly is, having automatic (that is, by mere will power) means of instantaneous adjustment of focus of the objective lens and of aperture of diaphragm or iris, to suit the distances and lighting of objects. In a camera these adjust-

ments are made by hand, by the diaphragms and rack-work. In looking at any open view we at first sight think that our eyes take in and register the entire view included in our angle of sight of about 120 degrees (see fig. 1), but a little examination will prove to us that this is not the case. It appears to us so because our vision—concentrated at any instant on one point only—travels with great rapidity over the salient features of the view, adjusting itself in focus and iris aperture to each point in turn. So rapid is this, that we are not conscious of it till we actually test the fact attentively. This is best done in the following way:—

Place yourself where several objects at different distances can be seen almost in a line with one another, but

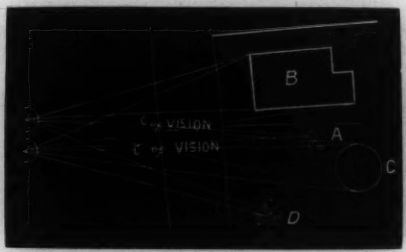

FIG. 7.

so that all these can be seen at the same time. Look first at the nearest object A (see fig. 6); keeping the vision on this object A you will see that B, C, and D are indistinct and double. Shift the centre of vision to B, then A, C, and D all appear double outlined and indistinct, and so on with the others. Repeating the experiment it will be found that the separation by double vision of each object becomes less and less towards D.

When looking at D, A appears in duplicate and widely separated, and when looking at A, D appears also in duplicate and less separated. This is due to the distance and to the narrow angle of the visual lines, and is shown in fig. 6, by the distances A′, B′, C′, D′, the apparent separation of the duplicated objects being represented

in the extreme cases of A and D, by the distances A′ and D′.

A camera lens takes in its entire field without concentration. It has a centre of vision, or optical axis, of course, but no power of concentration on any given point. How then do our eyes acquire this power? We can best show this by repeating the last experiment, but this time with one eye closed. We now find that there is no duplication of images, and, further, that we are still conscious of a centre of vision, and that this centre travels rapidly over the field of view; yet, there is less concentration on any point. One eye, in fact, sees the view just as the camera lens does, except that it has the power of focusing on any point, and, so far as there is concentration, it is due to direction and central focusing. It is evident, therefore, that the concentration in our ordinary vision is due chiefly in some way to binocular vision, of which the following is the explanation:—

FIG. 8.

When both eyes are directed on any single point, two complete views are received by the optic nerves, each one being seen from a slightly different direction, due to the separation of the eyes, and usually about two and a half inches. Every part of these two views, therefore, except the central spot, to which the eyes are directed, appears double outlined, because the two views only coincide correctly at the centres of vision. Figs. 7, 8, and 9 explain this. These diagrams are a plan, and two elevations of a house and garden, fence, two trees and a haystack. Both eyes are concentrated, that is, focused on the tree A, which, therefore, appears in the centre of both fields of view; but all the other objects will be double outlined in proportion, as they are remote from the centre A.

A is, therefore, the only object clearly seen, while the eyes are directed on it. Its two images, if not too large, will exactly superpose, being in the centre of the field, and may be represented by the sketch, fig. 9, exaggerated, of course, to show the double outlines. The view seen by the right eye is drawn in full lines, and that by the left eye in thin lines.

As the eyes are separated horizontally and not vertically, it is evident that this duplication of outlines is in the vertical and inclined lines, and does not affect horizontal lines, which, therefore, appear single. In double vision, therefore, all horizontal lines all over the picture or view appear clear and distinct to us, while the vertical and inclined lines are confused, except in the centre of vision. This is the reason that we think we see the entire view clearly, although, as shown in the foregoing experiment, we find on closer examination that only the centre of vision and horizontal lines are really perfectly

FIG. 9.

clear and sharp. If, therefore, we turn our heads down a little till the eyes are above one another we shall find the view before us is much more confused and indistinct, because in the majority of views the horizontal lines greatly predominate.

But we should here clearly understand how these pictures are received on the retina of our eyes, and, in fact, on the ground glass screen of the camera. We commonly suppose that the solid picture before us, consisting of objects at all sorts of distances, is represented as a plane on the camera screen and in the eye. We assume that the eye has no power of judging of the different distances from which the rays of light which reach it emanate, but this is an entire fallacy.

The lens in both cases reproduces the picture in the form of a small model of the visible parts of the actual one (inverted, of course) in which every object is only

in focus at its proportionate distance, as in fig. 8, and to see these different objects clearly as the eye travels over them, its focus instantly changes from one to the other, in fact, travels over this reduced image in the same way that the eye, in its centre of vision, travels over the visible parts of the actual objects. It is this faculty of automatic focusing which gives us the power of realising distances and solidity, that is to say, the eyes instantly alter the distance between the lens and retina as shown in fig. 9, to the dotted positions, when focusing on the objects they represent as to distance.

We must remember that the eye does not see the objects outside it, but only the image produced within it, and its rapid focusing is, therefore, also upon this inner image upon which all its adjustments are made.

Fig. 10.

This feature of the solid projection of objects by a lens seems to have been almost entirely ignored by writers on optics, and photographic projection; at least, I can find no reference to it, probably because all their theories have been directed to the production of plane pictures. It is certain, however, that our eyes adapt themselves to the solid projection of the picture, and, in passing over the image point by point—varying in focus—instantly change their focus and aperture to fit each point on which the centre of vision momentarily rests.

The photographer does the same with his camera screen and rackwork, but in doing so alters the focusing all over the picture. This explains also why the ground glass picture of the camera is so exceedingly beautiful and

life-like, and in full stereoscopic relief, whereas the photograph produced by the camera is comparatively flat.

This brings us to the value and use of duplicate or binocular vision. First of all, we see that all photographs, taken—as they are—with a single lens camera, ought to be viewed with one eye to see them as the camera saw them. And it follows, therefore, that if we shut one eye and look at any good photograph we get a view in relief, and the amount of this relief is exactly —allowing for the fact that the photograph is not so good as the view it represents—the same as we get with single

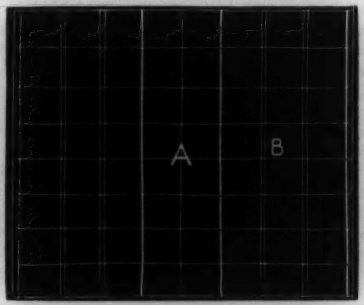

Fig. 11.

eye vision when looking at the scene itself, for the conditions are nearly the same; one eye, one lens, one view, unduplicated in outlines.

This variation in length of focus has been used in the Tacheometer, and many forms of range finders to calculate distances of objects. This fact seems also to explain the results obtained by the graphoscope, which is simply a large lens, whose diameter being greater than that of the width across the eyes, causes a photograph placed about a foot or so away to appear as if it were much larger and several feet distant (fig. 10), thus diminishing the duplication due to binocular vision, by

apparently placing the eyes further from the picture and reducing the visual angle.

We have found that clear vision is confined to a central spot, as at A, figs. 9 and 11, on which the sight always seems to be concentrated, and it is interesting to discover the size of this spot. At first sight we think it must be a large area, but a few trials will convince us that it is in fact exceedingly small.

Take an ordinary newspaper and hold it at the usual distance for reading—about 15 inches—and you will find your eyes travelling from letter to letter, and although each word is apparently taken in bodily, yet if you look closely at one letter of it you will be conscious that all the others are indistinct, which shows that at the distance of 15 inches the area of distinct vision is not as large as a pea. Similarly, by experimenting in the same way on greater distances, we shall find that at 4 feet distance this area is about the size of a threepenny piece, and for greater distances if we stick this coin on a window at 4 feet distance the area obscured by it on the middle and extreme distances is the area of distinct vision at those distances.

Fig. 11 shows the appearance of a window frame of small squares, as seen by the eyes, the centre of vision being for the moment at A. If the vision be concentrated at any other point, say B, then the vertical lines will coincide at that point instead of at A, and will be separated to the right and left of it.

We can now arrive at a reason for, and a true appreciation of, the advantages we gain from binocular vision. We gain in this way two projections of a view in the centre of vision, impressed or superposed on the optic systems in the manner shown in fig. 11, thus nearly doubling the intensity of the visual sense; and, further than this we gain the power of concentration of vision unknown and impossible in any single vision. It must be noted here that in single vision, with either eye, there is an apparent concentration of sight on the central area of the view.

It is due, however, to the fact that only this area is in focus, the eye being able to focus correctly only one point at a time, the margins of the field are fuzzy, in fact, this fuzziness of the view around the centre of vision is existent also in double vision, and accentuates the effect of

concentration of sight. If you fix your eyes upon any word of a printed page you will find that words an inch or two distant are so indistinct that you cannot decipher them. But the area of clear vision with one eye, is much greater than with two, though it differs in different observers, according as their sight is normal or degraded by age.

Having considered the fundamental principles of monocular and binocular vision, we come now to consider the theory of stereoscopy or solid perception, which is, undoubtedly, the most important element of sight, as it enables us to estimate distances and thicknesses of objects, and the picture before us, instead of appearing flat like a painting, appears solid and in substantial relief. Before starting any theory it is as well to try a few further experiments to illustrate this point, with one and both eyes.

Place yourself in front of any open view and allow the eyes to travel over it from foreground to distance. You will be conscious, in addition to the fact of concentration of vision on point after point, that the eyes alter their focus and aperture to suit each point on which they rest momentarily. This rapid adjustment of focus, which is the result of a life's daily and constant practice, is clearly the chief factor in the estimation of distances. In looking from one object to another at about the same distance the adjustment of focus is almost instantaneous, but in looking from a near object to a distant one, or *vice versâ*, the adjustment is very evident, taking an appreciable time to effect, frequently amounting to a second of time.

Other factors, called Criteria of Distances, that are essential to the estimation of distance, and solidity are atmosphere and the blue haze which increases and accumulates into the extreme distance, perspective, a knowledge of the real as compared with the apparent sizes of objects and the step-like gradations of terrace after terrace, field after field, or hill after hill, comprised in the view.

But Brewster and others assert that there is another visual sense introduced by binocular vision not accountable for by the above factors; that, in fact, with the convergent sight of two eyes we get two views, in the orthographic projection sense, of an object, the two views

enabling the right eye to see further round to the right and the left eye to the left of any solid object, by which it obtains an appearance of solidity not possible to single vision, and he explains the appearance of solidity of objects as being due solely to the convergence of the optical axes of the eyes on the various points of the object without any reference to the automatic focusing we have described.

That this idea is both incorrect and absurd is clear from our previous experiments, where we found that the area distinctly seen is too small to include any but very minute objects, whereas Brewster's idea could only be true if applied to the entire views of large and small objects at once. But we shall see this better if we continue our experiments.

Having examined the view with two eyes carefully, shut one of them, and go over the ground again, noting the differences. First, there is a considerable falling off of light, and consequently of distinctness. Then we note that the field of view is smaller, and that the part surrounding the clear area in the centre is fuzzy and out of focus, and that although the eye appears to travel over it as before, now the centre clear area is a much larger one. Thirdly, except that there is a loss of light, the distances are nearly as easily estimated as before, as also the solidity of objects. Fourthly, there are no double outlines, and the difference in definition between the central part and the margins is not so great.

A great deal has been made by the followers of Brewster of the fact that a person with normal eyesight, if called upon to use one eye only, fails to realise distances correctly, entirely overlooking the fact that he has been accustomed from infancy to use two eyes for this purpose, and that a man who has had only one eye for a long time, estimates and sees distances nearly as well as a two-eyed person. But to proceed. If you have thus carefully examined this one-eye view, then rapidly shut and open each eye alternately. If done quickly you will see that all the parts of the scene, except the centre of vision, move from left to right, and *vice versâ*, a short distance, due to the different angle of vision of the two eyes.

This experiment clearly establishes the fact that two distinct views are seen, and that these two views are not

the same, and will not superpose correctly, except in the horizontal lines and the centre of vision on which the sight is for the moment concentrated, and that, therefore, as the two images are not clear and distinct, they do not constitute stereoscopic vision. In fact, two images are not clearly seen at all, for the eyes only really see and take cognisance of the central area where the two images accurately superpose.

But there are many other cogent reasons against this Brewster theory of stereoscopy. Let us first reduce the last idea treated of to figures, viz., that with two eyes we see further round to the right and left of an object than with one eye. This, of course, can only apply to objects which can be seen round, and therefore does not apply to the majority of things that we see. Firstly, this can only be the case with vertical and inclined lines in the view, not at all with horizontal ones, as previously mentioned. Secondly, the larger the object the more stereo-

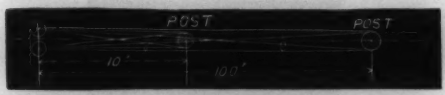

FIG. 12.

scopic it should appear, because we should be able to see further round it—which is untrue.

Thirdly, a cylindrical object placed vertically, and which therefore presents exactly the same appearance from any direction, should not appear stereoscopic at all according to this theory if solid perception is due solely or principally to convergence of the optic axes. But here, again, the theory is wrong. Fourthly, the further the object is from the eye, and therefore the less the angle of convergence, the less stereoscopic it should appear. This may have an apparent basis of truth in it, but it is due not to distance but atmosphere and reduction in size, for in exceptionally clear air, as on the tops of mountains, distant objects are seen as clearly in relief as near ones.

Now for a few figures on this point. If we stand, say, 10 feet from a post six inches in diameter, placed vertically, our eyes being 2½ inches apart, the two views we get of the post differ in the circumference of it by the .15

of an inch and the angle 0 is 3 degrees. (See fig. 12.) If we extend the distance to 100 feet the difference is reduced to .015 of an inch, and the angle is 18 seconds. These amounts are so absurdly small that it is preposterous to treat them as distinct views. The idea seems to have emanated from a study of orthographic projection or mechanical drawing, because it is necessary to draw a front and a side view of an object to show its solidity, though to the eyes an angle view, showing both sides at once, effects the same purpose.

But there is another difficulty in the fact that our eyes are separated horizontally and not vertically, so that the sensation of relief—if due to convergence of the optic axes—should be in the horizontal plane only. Thus a long, symmetrical article, such as a pole placed horizontally, would not appear in relief at all from horizontal

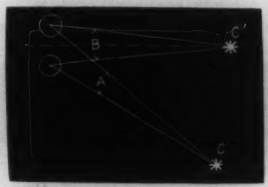

Fig. 13.

separation of the eyes. It is evident that this theory has been hastily conceived and too hastily accepted, probably on the reputation of its author, and also partly because it is certainly "catchy" in conception, but, as we have shown, will not bear examination.

There is no scientific basis, in fact, for the statement that two distinct and dissimilar views of an object are required to give a sensation of solidity, or that such distinct and dissimilar views will, in fact, effect this. The principles of optics give no support whatever to such a theory, but prove, as we have shown, that instead of relief a mere confusion of images must result, because two different pictures will not blend into one. There is no apparatus in our eyes to twist the lines of two discordant images into one concordant one, as Wheatstone supposed.

The convergent theory is easily disproved also by taking note of the angles of convergence. It is evident that, if true, for equal distances the angles of convergence must be equal, as in fig. 13. But if we look sideways at an object C without turning the head, the angle of convergence A becomes less than B—the front view C′, although the distance is the same in both cases, and appears to us the same also. This proves that the distance is discovered by focus, and not by convergence.

With the preparation due to the foregoing notes and experiments it will not be difficult to formulate the true theory of stereoscopic vision. First of all, we see things solid and in relief because they are so, and it would be singular if we did not so see them. The flat picture or view is purely artificial and artistic. It has been brought about by our attempts to portray natural scenery and objects on flat surfaces instead of in solid relief. Look at the picture thrown on the ground glass of a camera for comparison. It is not flat, but exactly as one eye sees it outside the camera. But the photograph we take of it is flat, because its colour, atmosphere, light, and shade, are either wanting or badly rendered, and, what is of the greatest importance, its detailed focusing is all reduced to one focus all over. (See fig. 10.)

Colour there is none, in fact; besides which the eyes cannot be deceived by a piece of paper, and therefore focus themselves on its surface, instead of on its various apparent distances. It looks much better to one eye, however, because the camera saw it with one lens. Therefore, with monocular vision, we are better able to focus the eye upon the distances represented in the picture instead of on its surface.

But if it needs two views of an object to give the sensation of relief, how is it that with one eye we experience any relief at all, if the convergent theory is true, and that objects seen with one eye to a person accustomed to using one eye, are just as stereoscopic, though lacking in illumination, and therefore in definition, as with two eyes? The alteration of focus of the one eye in conjunction with atmosphere and perspective accounts for its estimation of distances, as we have seen, but if the Brewster theory is right, must fail to give any sense of relief, because only one view of an object is seen instead of two,

and yet he admits, but without explaining, this property of monocular vision.

It is evident, therefore, that the two views of two eyes —that is, binocular vision—simply reinforce each other, doubling the illumination and definition of the picture in its concentrated centre of vision, confusing it at all other points.

Now let us see what we effect optically for the eyes in the use of a stereoscope. Our object in constructing this instrument is to place the eyes in reference to the view to be looked at as nearly as possible under the same conditions as they would occupy in looking at the original

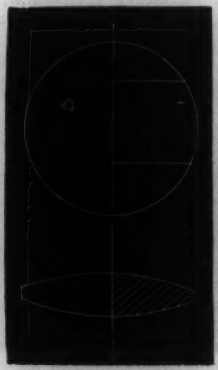

Fig. 14.

scene. These conditions are easily understood from what has been already advanced.

In the first place we note that, as a rule, no part of the scene viewed is nearer the eyes than, say, 10 feet, so that the angle between the eyes ranges from about 1 degree to infinity, that is to 0 degree. A stereoscopic camera has its lenses usually fixed for the latter position, that is, the optical centres are parallel, and about 3 inches apart, and two pictures are taken which differ from one another, as in the diagram, fig. 16. If these pictures

are finished as transparencies they can be placed one over the other, and the differences in vertical alignment noted.

Strictly speaking, the camera lenses should have their axes convergent to some point in the foreground in the absence of a power of varying their convergence, such as the eyes possess, and some cameras have converging lenses, which, however, produce distortion in the stereo-

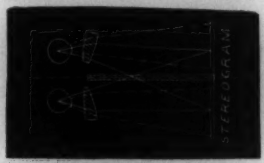

FIG. 15.

gram, because the latter is flat. The two pictures are reversed, that is, the left-hand print from the negative is placed on the right-hand side, and *vice versâ*, to correct the reversal of the photographic negative, and mounted about 3 inches apart, centre to centre, and the eyes view them through prismatic lenses, fixed about two and a half

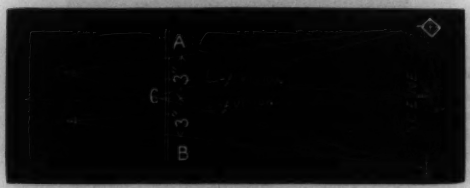

FIG. 16.

inches apart. These lenses ought to have means of lateral adjustment to suit the variation of separation of the eyes in different persons.

The prismatic lenses are, in effect, a combination of a prism and a lens, and are cut from an ordinary double convex lens, as shown in fig. 14, and both magnify and bend the rays of light which enter the eyes from the picture. The lenses and stereogram are placed, as in fig.

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15, with a partition between to cut off the light of the left-hand picture from the right eye, and *vice versâ*.

The effect of this arrangement, if properly carried out, is to place the eyes approximately in respect of the two views, in the same relation as occurs in viewing the scene in Nature, for in Nature the eyes take in pencils of rays, as in fig. 16 (the diagram is much shortened, and therefore the convergence of the eyes much greater than in actual fact), which, if intercepted on a transparent surface at A B, would show two copies of the view reduced and set side by side. That is, the left eye sees the view A C, and the right eye the view C B. We can easily effect this by sitting close to a window with the head steadied by a rest to prevent moving.

Then, if we shut the right eye and trace on the glass any part of the picture in front of us, to cover about

Fig. 17.

three inches square on the glass, we get the picture A C, and then if we shut the left eye and open the right one we shall find we can trace the same picture on a similar area C B of glass close beside the other, and thus get a rough stereoscopic view, and this double picture is what the stereoscopic camera is designed to take, so that when presented again to the eye in the stereoscope the two pictures will appear to the eyes in the same relative positions.

As these two views are blended on the retinas of our eyes in the manner described above, so in the stereoscope the two views stand in the same relation, and the eyes are satisfied with the illusion, so far as the views are good representations, and the centre of vision travels over the distances in the pictures in duplicate, with the same visual angle between them as in looking at the natural scene, and

not as though looking at the surface of the paper, as in a single picture (see the centre lines of vision, figs. 7 and 15, and compare them with those in fig. 17), which shows the visual angle in looking at a single picture where the eyes are focused on its surface.

Photographs have been taken frequently with a single lens camera, and two copies being printed have been mounted side by side as a stereoscopic view; such views are frauds, and although they show considerable relief, do not correctly register the sight of the two eyes, as they lack the slight divergencies of vertical alignment that occur, and to which the eyes are accustomed in binocular vision, and hence the illusion is not complete, and the eyes fail to refer the objects in such pictures to their proper distances.

It has just been said that in looking at correctly-rendered views in the stereoscope, the centre of vision of each eye travels over its own view in the same way as it would in viewing the original scene, and if you try the experiment you will be conscious also that the eyes focus themselves at distances suited to each object in the picture, as though these objects were real, and were situated at the distances they originally stood at, and this is the chief reason that such pictures appear stereoscopic

This change of focus in the eyes can always be felt if we are alert to take note of it. What we call straining the sight is usually forcing the eyes to an unusual or extreme focus, as in deciphering different objects at a distance, or in a bad light; and in looking at any and every kind of picture, which badly renders distances and objects, the sight is strained and forced into abnormal conditions of focus and visual angle.

Stereoscopy has suffered from the ignorance of the dealers who prepare and sell views. The great majority are very badly taken, badly mounted, printed and adjusted on the mounts, the result being that the eyes are strained out of the correct visual angle, and fail to appreciate the illusion. The ideal stereoscopic view should be one that is well and artistically coloured, that is, to represent Nature, not art with a capital A; for the purpose is to deceive the eye with an illusion, not to tickle it with an artist's fanciful creation. Hand-colouring is, however, generally very faulty, because of the difficulty of colouring the two prints exactly alike. When colour

photography is an accomplished fact, then we shall find one of its most successful applications will be to stereoscopic views.

Having thus a knowledge of the principles on which stereoscopy must rest, it is not difficult to devise various forms of stereoscope, and a good deal of attention is now being given to apparatus for giving the stereoscopic or solid perception to photographs, lantern pictures, etc., which will much increase the interest and beauty of these if successful.

Wheatstone and Brewster designed many forms of stereoscopes, reflecting, refracting, and lenticular, which it is not the present purpose, however, to describe. The only accurate one is the reflecting stereoscope, as others introduce, more or less, distortion and incorrect rendering of comparative distances. A simple means of viewing ordinary photographs, large or small, stereoscopically, would be valuable and popular if it could be done efficiently, without a cumbrous and expensive apparatus.

Discussion.

Mr. Dixey said that stereoscopy was one of the most interesting subjects one could possibly choose with which to approach the Optician. They were all under a considerable debt of obligation to Mr. Barber for having given them his views. His paper had been all the more interesting because he had approached the subject from a rather different point of view to that from which Opticians generally approached it. He had tackled subjects on which they as Opticians were bound to have opinions, such as how far stereoscopy was dependent on certain functions of the eye. Mr. Barber thought that stereoscopic perception was not at all dependent on the function of convergence. Personally, he thought convergence had more to do with it than any other simple function. There was no getting over the fact of the existence of a separate sense of plastic perception as distinguished from a reasoned appreciation of distance. He agreed with the lecturer that the function of accommodation had nothing, or very little, to do with it, but he thought that convergence had a great deal to do in stimulating the sense; but that the real controlling stimulus was the fact that with the two eyes the observer obtained two separate projections of the same image.

The lecturer had called their attention to a phenomenon that many of them had no doubt noticed, that when the eyes were shut and opened alternately, there was an apparent displacement of the two images; and he attributed this appearance to the difference in the point of view of the two eyes. Mr. Dixey, however, suggested that it was more often caused by a want of balance in the motor muscles of the eyes. The lecturer had also remarked that in looking through a stereoscope the eyes really focused themselves for the apparent distances of the different points in the picture. This Mr. Dixey could not accept; it would require an explanation as unnatural as the explanation of a miracle, he could only suppose that he had misunderstood the lecturer's terms.

Mr. Barber explained that Mr. Dixey had not misunderstood him, and that a full explanation of the "miracle" was contained in his paper.

Mr. Dixey, continuing, said he had a great idea that the thorough study of stereoscopy would have an important effect on the construction of optical instruments. He illustrated the interesting investigations that had recently been made by Professor Forbes and others in the application of the plastic sense to range finders. The notion of applying the principles of the stereoscope to range finding was a fascinating one. He feared, however, that absolute accuracy was unattainable; as in his view the personal error, which he believed to be mainly due to the want of muscular balance, would always affect the readings, however accurately the instrument was schemed.

Mr. Hopwood said that he had always regarded convergence as the chief factor in judging distance, which involved also the perception of solidity. Mr. Barber appeared to regard accommodation as the only means to this end. For this to be true in practice it would be necessary that everyone should be one-eyed. Convergence was, certainly, the great test of distance in vision with two eyes, but one-eyed people could, undoubtedly, learn to judge distances accurately by effort of accommodation, increasing that sense just as blind people cultivated extremely delicate touch. The effect of perspective as an aid to the mental estimation of distance and solidity should not be forgotten; its power was made evident by Messrs. Zeiss' new instrument, the

"Verant." Thus, it seemed that one-eyed persons could perceive solidity, but probably by an entirely different process to that of binocular vision. The "plastographs" shown were originally known as "anaglyphs," and were invented by Ducos du Hauron.

Dr. Glazebrook, on rising, thanked Mr. Barber for his very interesting paper. Some of the experiments and illustrations reminded him of old days at Cambridge and lectures there. He thought Mr. Barber had given them a great deal to think about, and regretted the fact that it had not been possible to have the paper printed and in the hands of members of the Society. He thought it would be very interesting if he could tell them a little more definitely something about the changes that might go on in the eye when the distance of the central point of fusion was slightly varied.

Mr. Barber had put down almost the whole of the stereoscopic effect to those changes. Dr. Glazebrook thought that that was a point on which they ought to be able to experiment and obtain satisfactory results. He was very much interested in the figures given by the lecturer when speaking of the other method of looking at the problem, showing how very slightly different were the two pictures seen by the two eyes in the cases he mentioned. However, he did not feel sure that Mr. Barber had upset the old view put forward by Messrs. Dixey and Hopwood, that convergence had a very great deal to do with our sense of distance.

Mr. Barber, in reply, observed that Mr. Dixey and Mr. Hopwood had stated that they were unconvinced so far as the question of convergence was concerned, but, unfortunately, they had not shown any reason for their views. He, on the other hand, had given very strong reasons why convergence could not be the cause of stereoscopic effect, and not one of his reasons had been tackled. Mr. Barber explained his point by referring to the appearance of a column. Could Mr. Dixey explain how it was that he saw the convex middle of the column standing out?

Mr. Dixey stated that it was impossible to conceive a column perfectly lighted. One saw it, not as a pure subject, but as one which was shaded. There were also the surroundings which acted unfavourably.

Mr. Barber, continuing, said the other point Mr.

Dixey referred to was that some muscular action of the eye caused the fusion of two images. That had been suggested by several writers on stereoscopy. If there were any such elastic movement of the retina at all, distortion would be introduced. As a matter of fact, however, the image thrown on the eye was the most accurate production of any optical instrument known. He did not think that convergence had anything to do with stereoscopic effect. There was no doubt, however, that the criterion of distance had a very great deal to do with stereoscopic effect. He considered that fig. 13, and the fact that convergence could have no influence on horizontal lines and objects, were unanswerable.